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THE "MUSCULAR SENSE"; ITS NATURE AND CORTICAL LOCALISATION.[1]

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HAVING been honoured by a request from the Council of the Neurological Society to open a debate upon "The Muscular Sense; its Nature and Cortical Localisation," I willingly undertook this duty, not because I thought the task an easy one, but, recognising to the full all the inherent difficulties of the subject and the unsettled state of opinion thereon, I fully concurred in the notion that such a discussion might do much good. If it does not lead to the immediate settlement of the many points still in dispute, it may, at least, serve to bring out into clearer light the nature of the problems to be settled by future workers and thinkers, in order that unanimity may ultimately prevail. I say "thinkers" advisedly, because this is eminently one of those subjects on which observation and experiment alone will not suffice, especially observations and experiments conducted upon lower animals.

INTRODUCTION.

It may be regarded as a physiological axiom, that all purposive movements of animals are guided by sensations or by afferent impressions of some kind.

Physiologists know that the several kinds of movements

[1] Paper read before the Neurological Society of London, on December 16, 1886.

ERRATUM.

On page 234 of the present Volume, the last paragraph of Case I., beginning with the words, "This gland," etc., is the concluding paragraph of Case II., and has been accidentally transposed.

the idea or revived sensory process occurs, and the particular motor mechanisms connected therewith, whose excitation gives rise to the fitting movement.

But simple, familiar, voluntary movements are attended by so little conscious illumination as to be often indistinguishable in every way from ideo-motor movements. The utterance of words in ordinary speech, that is, in the person's own language, is effected by movements which may be described either as voluntary or as ideo-motor. The whole act of speech is truly a voluntary performance; but the enunciation of each word takes place with all the ease and lack of attention typical of an ideo-motor movement. It is, however, in the performance of a new or unfamiliar voluntary action that we find the widest divergence from the phenomena attendant upon the execution of a simple reflex movement. Here we have the full blaze of consciousness, the whole attention of the performer, directed to the production of the movement in the desired manner, though this result may not be attained till more or less marked failure has occurred on many occasions. What can guide these endeavours to perform new movements save sensory impressions, actual or revived? What is the result of this concentration of attention upon present and revived sensory processes? This latter question cannot be answered definitely and in detail, but seeing that one result of practice is to make the new movement after a time quite easy of execution, and that further practice still causes it to recur with such facility that it may be fully entitled to take rank as an 'ideo-motor' or even as a 'secondary automatic' movement, we are fairly entitled to conclude, that one of the hidden effects whose existence we can only infer has been the laying down of beaten paths for efferent stimuli, from the sensory centres which have been concerned in guidance (the stimuli in question being in part, at least, the molecular movements occasioned by the revived activity of the sensorial centres) to the particular combinations of motor nerve mechanisms whose excitation is needful for evoking the movements in question.

But if it be assumed that this is what occurs when, by dint of long practice, an at first difficultly-executed voluntary movement becomes, in process of time, a movement so easy of execution as to recur independently of conscious attention,

which animals are accustomed to perform are only artificially and for purposes of convenience divided into such classes as (1) reflex or automatic, (2) secondary-automatic, (3) instinctive, (4) ideo-motor, or (5) volitional.

From the point of view of the dependence of these several kinds of movement upon sensation, or upon afferent impressions either actual or revived, no fundamental distinction can be drawn between them.

In reply to this it might be alleged that volitional movements constitute a class apart, seeing that their performance implies as a necessary factor, which the others do not, the presence of, and illumination derived from, Consciousness. But it must be borne in mind, (*a*) that Consciousness is not itself a factor, it is rather of the nature of an epiphenomenon; (*b*) that it exists in varying degrees during the executions of different kinds of voluntary movements, being of maximum intensity during the performance of unfamiliar muscular actions and of minimum intensity (almost vanishing) in association with easy and familiar voluntary acts; and (*c*) that as a matter of fact, it is often almost impossible to draw anything like a dividing line between ideo-motor and voluntary movements, so insensible are the gradations between them.

If we compare for a moment a simple reflex movement with a volitional movement, some important differences may nevertheless be found, which are of great significance from the point of view of the subject with which we are now concerned.

A reflex movement may be evoked and guided to its completion by afferent impressions which are wholly unfelt; we have here apparently nothing else than a direct relation established between the paths of afferent impressions and the nervous cell and fibre tracts needful for evoking the suitable activity of some definite sets of muscles. In other words, the line of least resistance for the molecular movements set up in the cells on the afferent side is found to lie along the intermincial fibres connecting these with certain groups of motor cells, and thence outwards through their efferent nerve fibres to the muscles. Even in ideo-motor movements something of the same kind seems to occur; that is to say, there seems to be an equally well-formed beaten tract (by means of nerve fibres) between the cells of the perceptive centres in which

with machine-like regularity, three important consequences follow:—

In the first place, it seems to afford an organic proof that even as in ideo-motor actions, so in voluntary movements, the immediately evoking stimuli flow out from the sensory centres concerned with the production of such movements—the voluntary movement grows into the ideo-motor movement, and then unmistakeably the guidance is wholly through the sensory centres, for then (as we have seen in the case of a simple reflex movement) the line of least resistance for molecular movements from the sensory cells is through certain new communicating fibres bringing them into relation with definite groups of motor nerve mechanisms.

Secondly, it seems clear that the same motor mechanisms must be concerned with the production of the new voluntary movements as with the ideo-motor or secondary automatic movements into which they become converted at a later date. We have to do in these two sets of cases with differences in the degree of perfection of the nervous mechanisms, and with different degrees of perfection in the functional and structural relations between the sensory and the motor mechanisms especially, rather than with an alteration in the position in which the movements are, so to speak, organised. It is, therefore, in my opinion, a fundamental error to look for special voluntary motor centres. The strictly motor side of voluntary motor mechanisms should be identical with, or lie side by side with, the strictly motor side of "ideo-motor" or "secondary-automatic" mechanisms of the same type.

Thirdly, we may safely conclude, that when once the ways or beaten tracts have been laid down by which certain sensory departments are connected with certain motor mechanisms for the performance of voluntary movements, the former may be incited to renewed (or ideal) activity, and may be followed by appropriate responsive movements, almost if not quite independently of, and certainly without any need for, Consciousness as an attendant of such revived sensorial activity: in other words, when once the internuncial tracts have been clearly established from cortical sensory centres to motor mechanisms, it is not in the least necessary that Consciousness should be an appanage of this revival, since even the unfelt recall by associa-

tional processes of activity in the guiding sensory centres, may by causing an outflow of molecular movements, now suffice with machine-like regularity to evoke the accustomed motor reactions, as in the case of a skilled dancer or a skilled player upon a musical instrument.

Sensations Resulting from Movement: Kinæsthetic Impressions.

Before entering into any details concerning the sensory guidance of movements in general, something requires to be said about one particular class of sensory impressions which are of great importance in this relation. I refer to the body of sensations which result from or are directly occasioned by movements. This constitutes a complex of impressions which, for the sake of convenience of reference as well as for the purpose of indicating their common functional relations, I have proposed[1] to include under the designation *Kinæsthesis*, or the "sense of movement," perhaps I ought rather to have spoken of "sensations of movement," as Wundt does,[2] but the former term is more in accordance with those already in use in connection with this subject (e.g., "muscular sense," and "sense of force"). Impressions of various kinds combine for the perfection of this "sense of movement," and in part its cerebral seat or area corresponds with the sense of touch. Thus, under it are included, as its several components, cutaneous impressions, impressions from muscles and other deep textures of the limbs (such as fasciæ, tendons, and articular surfaces) all of which yield conscious impressions of various degrees of definiteness; whilst, in addition, there seems to be a highly important set of unfelt or but little felt impressions which guide the volitional activity of the brain, in ways hereafter to be defined, and which serve to bring it into relation with the different degrees of contraction of all muscles that may be called into action.[3]

[1] 'The Brain as an Organ of Mind,' 1880, p. 543.

[2] Wundt says:—"Les sensations du mouvement, comme nous avons montré précédemment, sont pour nous *des produits fusionnés complexes*, provenant de sensations d'origine différente" ('Élém. de Psych. physiol.' Trad. franç., 1886, i., p. 421.

[3] I have introduced a qualifying phrase after the word "unfelt" above, because

By means of this complex of sensory impressions we are made acquainted with the position and movements of our limbs, we are enabled to discriminate between different degrees of "resistance" and "weight," and by means of it the brain also derives much unconscious guidance in the performance of movements generally.[1]

Of course it may be easily said, and doubtless will be said, by those who have not yet realized the importance of bestowing special attention upon this complex of impressions, that it is unsuitable to describe as though it were a distinct endowment, the means by which we receive and appreciate a group of impressions having such diverse sources of origin. This mere technical or formal objection seems to me, however, to have little weight, when we consider that from a functional point of view they constitute a group of impressions altogether apart, recurring with extreme frequency, and always in more or less similar combinations, during almost every hour of our waking life—subserving also, as we hope hereafter to show, a most important purpose. I think, therefore, we are warranted in speaking of the reception of this group of sensations under the name of kinæsthesis, or the sense of movement. There will certainly be a real convenience in being able to allude to such impressions briefly, as 'kinæsthetic impressions.' It is confessedly a mixed group partly "intrinsic" and partly "extrinsic" in their origin.

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on reflection I think I have been wrong hitherto in saying that "muscular sense" impressions are wholly unfelt. There must be a kind of consciousness associated with them if, as seems now to be established, our knowledge of the position of our limbs, as well as of differences in weight, is in the main due to "muscular sense" impressions.

[1] These impressions are almost always closely linked with others of a tactile, visual, or auditory order, so that their associated ideal recall presents no sort of difficulty, and the question whether, when recalled, they reveal themselves in consciousness or not is one which has no sort of importance for, as Ribot says, consciousness "n'est cause de rien." Writing in the 'Revue Philosophique,' June, 1884, p. 639, L. Manouvrier pointedly illustrates this in the following manner: "En son absence, les cellules cérébrales peuvent être le théâtre d'opérations très parfaites auxquelles peuvent succéder des mouvements très bien appropriés et coordonnés, tout comme s'il y avait eu production de conscience. La seule différence est que le cerveau sert alors de substratum à ces opérations nerveuses sans le connaître, tandis que, dans l'état opposé, il les subit consciemment, voilà tout."

From one point of view, especially, important differences separate this synchronous and fused group of impressions from all other sensory impressions—differences to which it will be as well at once to allude.

Ordinary extrinsic sensorial impressions commonly play the part of instigators to movement. Thus impressions of smell, sight, hearing, touch, and taste are either the immediate or the remote instigators of the great majority of the movements executed by animals.

The movements so stimulated are in almost all cases of a more or less purposive type, directed, that is, to the attainment of some end.

During the whole time that such movements are being evoked the animal is constantly receiving those groups of mixed afferent impressions which we have styled "kinæsthetic," called into existence by the mere movements themselves.

Kinæsthetic impressions are never instigators of movement in the same sense that olfactory impressions may be instigators of movement. Visual impressions and auditory impressions are not only frequent instigators of movement, they are also respectively all-important guides for certain classes of movements. From a functional point of view, kinæsthetic impressions are guides only; these functional uses are, however, co-extensive with movements themselves, since in conjunction either with visual or with auditory impressions they act as guides for all sorts of movements. As it is with visual and auditory, so is it with kinæsthetic impressions, their guiding influence in the production of movements is brought to bear partly under the form of actual sensations and partly under the form of revived impressions (the memories of past activity).

Seeing that kinæsthetic impressions must be as numerous and as endlessly diversified, in their kinds and combinations as movements themselves, it follows that large tracts of the brain ought to be concerned with their registration for future use in the guidance of all kinds of voluntary movements.[1]

[1] I purposely leave out of account for the present the fact, that kinæsthetic impressions enter largely into the composition of nearly all our visual and tactile impressions; movements of the eyes, or movements of the hands and fingers, not only vastly increasing the range of possible visual or tactile impressions, but also

Except during the last forty years, the occasional references which have from time to time been made to kinæsthetic impressions have appeared rather in the writings of philosophers, than in those either of physiologists or pathologists. Yet the first mention of such impressions goes back, according to Sir William Hamilton, to a rather remote past. He tells us that two Italian physicians, Julius Cæsar Scaliger, in 1557, and Cæsalpinus of Arezzo in 1569, quite independently of one another, were the first to recognize and definitely state that the exercise of our power of movement is the means whereby we are enabled to estimate degrees of "resistance," and that by a faculty of "active apprehension" which was by them contrasted with touch as "a capacity of sensation or mere consciousness of passion." These early references to such an endowment, like those of Maine de Biran and Sir William Hamilton himself, have for the most part supposed the existence of a separate faculty or endowment associated with volition, or the mere will to move, to which such names as the "sense of effort," the "sense of force" or the "locomotive faculty" have been given—agreeing very closely with what Wundt has since termed the "sense of innervation."

Some of the causes why physiologists should for so long have paid but scant attention to this class of impressions may be thus enumerated:—

(1) The most typical and important group of kinæsthetic impressions, viz., those derived from the muscles themselves, belong like visceral impressions, to the class of "intrinsic" sensations, which, as a whole, receive but little of our conscious attention under all ordinary conditions of life. These are "means" rather than "ends," and consequently soon become sub-conscious modes of activity.

(2) These impressions are not directly concerned with the organism's life of relation—that is, they do not directly

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entering as it were into their very structure, and thereby enabling us to recognise and judge what philosophers call the primary qualities of matter, viz., solidity, extension, figure, etc. I also omit further mention of the fact, that kinæsthetic impressions likewise act as guides in the great class of reflex movements which are produced independently of the cerebral hemispheres, by the mesencephalon and cerebellum, as well as by other sensori-motor couples of lower grade.

stimulate its converse with the outside world. Impressions derived from movement cannot in themselves be, nor are they when ideally revived, first-hand instigators of movement, however much they may aid in the repetition of movements previously effected.

(3) Owing to the nature of this sensory endowment, it is one which cannot be investigated by experiments upon animals. The presence or absence of ordinary sensory endowments may be tested in the lower animals, by looking for the recurrence or not of some particular motor reactions which we know are apt to be excited by the presentation of this or that stimulus to the sense which is being tested. The sense of movement being however a so-called "intrinsic" sense, and not an instigator of movements, cannot be tested in this manner. Indeed it may be safely said that no experiments that can be made upon the lower animals, even upon monkeys, are capable of throwing any decisive and direct light upon the presence or absence of the most important element in this endowment, viz., the impressions emanating from contracting muscles.[1]

The Muscular Sense.

It should be stated here that the announcement of the existence of a "special class" of sensations emanating from muscles, came from Sir Charles Bell. He first postulated the existence of a special endowment, to which he applied the name "*muscular sense*," whilst the reality of the sensations emanating from muscles was first demonstrated and accurately estimated by the experiments of E. H. Weber,[2] who, however,

[1] Thus it happens, perhaps, that the evidence on which Ferrier relies in support of the notion, that the "muscular sense" was defective in three monkeys operated upon by him (*See* 'Functions of the Brain,' 2nd ed., 1886, pp. 335, 340, 344), is no more convincing to me, than is the evidence of Hitzig and Nothnagel to him, in support of their cortical localisation of muscular sense centres (p. 379). We must have observations on men and women for the solution of this problem, who can *tell* us what they know of the position of their limbs, and of their movements (active or passive), as well as concerning their ability to appreciate differences in weight.

[2] Annotationes anatom. (Progr. collecta.) Proleg. xii. (1831). *Tastsinn und Gemeingefühl.*

spoke of the endowment by which we are enabled to discriminate different degrees of "resistance" or weight, as the "sense of force." E. H. Weber was the first to make definite investigations as to the differences in weight which could be appreciated by the mere tactile sense of contact and pressure whilst the hand was at rest on a flat surface, and, subsequently, to ascertain by careful experiments that a considerably heightened power of discrimination existed when the muscles were called into play—that is, by no longer allowing the hand to rest upon a flat surface, but by slowly moving the arm when different weights were successively placed upon the palm. Numerous comparative experiments of this kind made by Weber, and since confirmed by other investigators, have shown, that the activity of the muscles adds greatly to our ability to discriminate differences in "weight" or "resistance." Thus, by the mere sense of cutaneous pressure it has been found that the addition of one-third of the original weight, whatever it may have been, is needful to produce a perceptibly different impression; while, on the other hand, when muscles are allowed to be called into play the power of discrimination is so greatly heightened that an addition of no more than $\frac{1}{17}$th of the original weight is generally capable of yielding a distinguishable difference in the resulting impressions.

This constitutes, in fact, the physiological proof of the existence in us of a distinct ability to discriminate different degrees of weight or resistance by impressions resulting from various states of tension or contraction in our muscles. This ability is lost in certain diseases, and its absence gives rise to disordered movements when the eyes are closed, as well as to ignorance of the position of the limbs and other related defects.

For the better understanding of this part of our subject it seems best to record here, in sequence, the different cases to which I desire to call attention, for the purpose of illustrating the nature of the defects themselves, as well as the nature of the diseases with which they are most commonly associated.

As samples of this kind of evidence I will first cite a few details (all that I have been able to find) concerning two cases which were reported at a time when no particular attention had been given to such morbid phenomena.

"In February, 1790, a German schoolmaster, after extreme worry, was seized with an insensibility extending over the whole body. Movements were freely executed at all the joints, and all his muscles were entirely under the influence of the will, though the patient had no feeling of having executed movements. He could move his toes separately or together at will, but in order to know whether the movement had really taken place he was obliged to assure himself by means of his eyes. He walked quite easily, though it seemed to him as if he did not make use of his own legs."[1]

Demeaux[2] gives details concerning a woman suffering from hemi-anæsthesia, which show the complete loss in her of the sensations coming from muscles, as well as of all other modes of sensibility, superficial and deep, in the affected limbs. He says:—"She put her muscles in action under the influence of her will, but she had no consciousness of the movements which she executed; she knew not what was the position of her arm—it was impossible for her to say whether it was extended or flexed. If one told the patient to raise her hand to her ear, she executed the movement immediately; but when my hand was interposed between her own and the ear, she was not conscious of it; if I stopped her arm in the midst of its movement, she did not become aware of it. If I fixed, without allowing her to be aware of it, her arm upon the bed and told her to raise the hand to her head, she strove for an instant and then became quiet, believing that she had executed the movement. If I induced her to try again, showing her that her arm had remained in the same place, she attempted to do so with more energy, and as soon as she was compelled to call into play the muscles of the opposite side [of the body], she recognized that the movement was opposed."

In these cases the so-called "muscular sense" was clearly absent, and so were all kinæsthetic impressions in the affected limbs, leading to an ignorance of their position and movements. Details will now be given concerning another very remarkable case, of a somewhat different type, which was examined with great care and reported at length by Landry,[3] in his interesting "Mémoire sur la paralysie du sentiment d'activité musculaire."

The patient was a physician, fifty-five years of age, who, from Landry's account, I judge to have been clearly suffering from

[1] Reported in Hufeland's 'Journal,' and quoted by Bellion in his 'Recherch. sur la Pathol. et Physiol. des Sens. tactiles,' Thèse de Paris, 1853, p. 46.

[2] 'Des Hernies Crurales.' Thèse de Paris, 1843, p. 100.

[3] 'Gaz. des Hôpitaux,' 1855, Obs. III., p. 270.

locomotor ataxy of an unusual type. In regard to his power of executing movements with his eyes open and closed respectively, and as to his knowledge of his movements and of the different degrees of tension in his muscles, the following quotations may be made:—

"Voluntary contraction seemed to have lost nothing of its energy. When seated or in the lying posture he easily elevates his lower limb and maintains it elevated for a long time without apparent fatigue, only the limb is the seat of oscillations which he always in part controls when he sees them. He has no idea of the more or less extensive and violent passive movements which his limbs are made to undergo. Active movements are not even appreciated as soon as he ceases to watch them. Thus, after having made him close his eyes, if he is asked to move one of his lower limbs in whole or in part, he does it, but cannot say whether the movement executed is great or small, energetic or feeble, or even whether it has really taken place; and when made to open his eyes whilst he is moving his leg from right to left, for example, he declares that he had only a very inexact idea of this action. In the same way if after having specified to him some aim to be accomplished, and having made him measure the distance with his eye, he is told, without looking, to bring his foot to the spot, it remains on this side of it or goes beyond without his suspecting it, or if he succeeds it is only by chance. On the contrary, when he watches the movement of his foot he easily places it correctly. . . . In fact, when he cannot see his limbs, he is no longer conscious of their existence—he is ignorant of their position. When in his bed he loses them, so to speak, and has to search for them with his hands. In the sitting posture, he often seeks to extend or to flex them when they are already in these positions. If, having the wish to execute a movement, I prevent it, he does not perceive it and attributes to the limb the position which he had intended to give to it. He neither appreciates the resistance one opposes to his movements, nor weights with which one charges his limbs. He easily raises a very heavy object when he has estimated its weight by sight; but if he is not permitted to judge of this previously, he does not know how to apportion his effort to its weight, and the muscular contraction is either too great or decidedly too little.

"In the upper limbs there are almost the same disorders: ill-co-ordinated and badly measured movements; defective appreciation of passive movements, and even of active movements when the patient does not see them; but faculty of co-ordinating and of measuring his movements when he calls to his aid his sense of

sight; great energy of muscular contraction; integrity and rapidity of each isolated movement of the fingers, of the hands, of the forearms, and of the arms; impossibility of appreciating weight or resistance; sensation of extreme lightness of the hands, etc. Prehension is much altered; the patient seizes an object clumsily, and only succeeds in seizing it better when he attentively watches his fingers. He can no longer write. As soon as his attention is relaxed he lets objects fall which he may have been holding, without perceiving that their weight has ceased, or that his fingers are extended. Everything appears to him without weight. He can neither appreciate the thickness nor the size, nor the form of bodies which one makes him touch, or rather almost always attributes to them a form and a size almost identical—he describes them as cylindrical or round, and of about two centimètres in thickness. If, after having made him close his eyes, one takes from his hand an object which he is holding, he does not perceive it, and continues to maintain the position he had taken in order to hold it, believing that he feels something of the same shape and volume. The same thing happens if, equally without his knowledge, one has completely opened his hand; he believes it to be still closed and filled by an object whose form and size he judges as if he had never ceased to hold it. He does not perceive the resistance which is opposed to the movements of his fingers and hand, and often thinks that he has executed a movement when he has been prevented from doing so. He cannot judge of the consistence of bodies, to which he attributes without discernment either hardness or softness. When one plunges, without his knowledge, one of his fingers in water, he perceives the coldness very well, but when he is asked to move his finger about, he thinks he moves it in air and does not perceive the resistance of the liquid.

"His movements have, as I have said, a great irregularity; but all, even the most delicate movements of the fingers, are executed with the greatest facility. When he looks at them one can only prevent their flexion with difficulty, and he can press one's hand vigorously. If he ceases to see, he continues to make efforts to flex his finger or his hand when opposed, but he does not know how to apportion his contraction to the resistance; he also presses the hand very softly, as if there were a real loss of power. With his eyes open, he easily opposes the thumb to each of the other fingers; with his eyes closed, the movement of opposition occurs, but the thumb only by chance meets the finger which it seeks. With his eyes open he is able, without hesitation, to bring his two hands together; but when his eyes are closed his hands seek one another in space, and only meet one another by chance.

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The fingers can no longer be regularly interlocked. There is not the least tremor of the hand."

"*Condition of cutaneous sensibility in the four limbs.*—Sensations of contact preserved but obtuse; M. X. feels quite well when he is touched, but he cannot say with what kind of object. Thus he cannot decide whether it is with wool, with wood, or with the observer's hand. He no longer feels tickling. He is able perfectly to specify the precise point at which he is touched, unless the body with which the contact is made covers only a very small area of skin, as, for example, the blunt point of a needle. Sensations of temperature intact; sensations of pain much exaggerated."

Landry gives two other cases very similar to the above, but I will next quote a few details concerning a very remarkable case which, in one important respect, stands in marked contrast with those that have gone before, and more especially with the two cases which will follow—I mean, as regards the patient's ability to execute precise movements when the eyes were closed. This is a case in which a more or less general anæsthesia existed for over thirty years, and the man was repeatedly examined by many trustworthy observers. The record of the case was published by Spaeth,[1] and the autopsy was fully described by Schueppel.[2]

Remigius Lens, aged forty-two, in the year 1862, has suffered for twenty years with anæsthesia of the hands and arms, which rapidly became severe; for six years similar troubles have existed in the lower extremities. Present condition: upper extremities wholly anæsthetic; on the soles of the feet the sensations of touch, pressure, and pain are entirely extinct, and in the legs considerably diminished. *He falls when his eyes are closed.* In the dark, when in bed, he feels as if floating in the air, as the anæsthesia extends to the trunk.

March, 1864.—Sense of pressure in the upper extremity, and the sense of force, entirely extinct. Sense of position of the upper extremity and of passive movements of the latter completely extinct. Movements of the upper extremity powerful and perfectly correct; the patient eats alone, dresses himself, etc., as far as he can direct his acts with his sight. When the eyes are closed, the hands are moved nearly like those of a blind man. In the lower extremities, besides the cutaneous anæsthesia, there is

[1] 'Beitr. zur Lehre von der Tabes dorsalis,' Tübingen, 1864.

[2] 'Arch. d. Heilk.,' Bd. xv., 1874, p. 44.

complete loss of the sense of passive movements and of the position of limbs. In spite of this the patient can walk without support, quite fast and securely, for a good distance. If he is asked to raise his foot to a given height while his eyes are shut, he accomplishes the act by a perfectly quiet and suitable motion.

June, 1872.—Sensibility continues the same. When the eyes are shut he has no idea at all of the position of his limbs, and, if standing falls. He can still walk clumsily, but not atactically. He can perform all desired actions with his arms, as long as he can see them.[1]

This patient seems to have had decidedly more power of executing accurate movements when the eyes were closed, than is commonly met with in such cases. But then the anæsthetic condition of the arms had lasted for twenty to thirty years when the results above recorded were obtained, so that ample time had elapsed to permit his nervous system to accommodate itself to its altered conditions, and so to gain a power of executing voluntary movements with closed eyes, which probably did not exist at first in this patient, in whom "muscular sense" impressions appeared not to reach the cerebral cortex, however it may have been with lower centres. This is certainly an altogether exceptional case.[2]

Duchenne's attention seems to have been attracted to the effects of loss of the "muscular sense" very shortly after the first cases were published by Landry.[3] A short paper from him, entitled "Usages de la sensibilité musculaire" appeared in 1853,[4] which two years afterwards was reprinted and added to in his work "De l'électrisation localisée" (p. 410). Here Duchenne points out that such cases as I have above referred

[1] Abstract quoted from translation of Ziemssen's 'Cyclopædia,' vol. xiii., p. 89.

[2] Yet we find Dr. Ferrier relying upon it, in spite of all other evidence to the contrary, apparently as sole support of a view, which will assuredly find no general acceptance, to the effect that a "limb can be moved freely and forcibly and directed without uncertainty, for volitional purposes," when the eyes are closed, "in the entire absence of any sense of movement" (*loc. cit.*, p. 65). In regard to that point I would refer him to the cases of Landry, as well as to those now about to be recorded from the observations of Duchenne, Briquet and Bazire (pp. 16–20).

[3] Archiv. génér. de Méd., 1852. Other cases were recorded in 1853 by Bellion (Thèse de Paris) 'Rech. histor. sur la pathol. et la physiol. des sens. tactiles.

[4] 'Moniteur des Hôpitaux.'

On devoting myself to the examination of this patient I found that she presented in the highest degree the phenomena which constitute the special object of this article. Thus having stooped in such a manner as to prevent her seeing her hand at the moment when I told her to close it, the hand remained motionless though she thought she had closed it; and, whilst I was making her extend and flex the forearm upon the arm, having turned her attention away from this limb the movement was immediately arrested—the limb remaining in position as if it had been tetanised. I should add that the patient, whom I had seen for the first time, was quite ignorant of what I sought to establish. . . . But here is another experiment which shows, in addition, that the action of the will in combination with the sense of sight was necessary also for the cessation of a contraction once produced. If after having made her squeeze the hand, she is prevented from seeing it, and told to cease all effort, one feels that the contraction of the flexors continues, and it is necessary to employ considerable force to open her hand. Or, if after having made her flex the forearm, one prevented her seeing it the forearm remained in a state of flexion, and it was necessary to employ a pretty strong force to extend it (it must be recollected that the patient, deprived of all sensibility, had no consciousness of the movements which were impressed upon her limbs").

"Here, again, is another experiment which shows that it is not sufficient for the patient merely to be able to see in order that the movement should be obtained, but that it is also necessary for her attention to be fixed upon the limb about to be called into movement. Having placed the patient's hands sufficiently close to one another so that she was able to see them both equally well, I asked her to shut them and to open them both at the same time. The flexion was executed, but alternately on the two sides; and it was the same in regard to the extension. She was not able to contract homologous muscles at the same time, whatever effort she made to obtain such a result. One could see that during the contractions she alternately fixed her attention upon the hand about to be called into movement. It was no longer possible for her to flex or extend simultaneously both forearms." This patient remained under observation for many months without any notable change in her condition.

The account given by Duchenne of this remarkable case was subsequently entirely confirmed by Briquet. He also holds that this extreme condition of anæsthesia of the muscles

to (excepting the last, which, of course, appeared after the date of his work) constitute only one category of those in which there is paralysis of what Landry termed "le sentiment d'activité musculaire." He says:—"In the second category, which is much less numerous, *the patients when similarly deprived of sight lose the faculty of executing the simplest voluntary movement.* If they are bid for example, to open or shut the hand, flex or extend the forearm, in a word, whatever movement one asks them to make, the muscles which ought to enter into contraction remain inert, notwithstanding all the efforts of the will. One only observes sometimes certain irregular movements, slight in extent, weak, and different in kind from those that they wish to execute—movements of which they have even no knowledge. Nothing can describe their astonishment when they perceive, after the experiment, that their limb has remained in the same situation, when they thought they had made it execute a movement. Their surprise is all the more great, since they can execute this same movement with rapidity the instant that they are permitted to look at the limb."

In confirmation of these statements Duchenne gives a detailed account of a patient in 'la Charité' under the care of Briquet, whom they conjointly submitted to repeated careful examinations.[1] The patient was an hysterical girl who was suffering from complete anæsthesia, superficial and deep. She presented all the same kind of phenomena as were seen in the cases already recorded, that is to say a lack of knowledge as to whether a movement which had been willed had been executed or not (necessarily, therefore, an ignorance of the extent of her movements and of the position of her limbs), together with a total inability to appreciate degrees of resistance. In addition, however, there were new peculiarities concerning which the following quotations may be made.

"There was also another phenomenon which M. Briquet and I observed in this case of general and profound insensibility: that is, one could violently strike her limbs, shake them violently, and change their place, without her having any knowledge of it. . . .

[1] *Loc. cit.* p. 417.

case recorded by Bazire,[1] very similar to those reported by Duchenne and Briquet. Only here we have to do with hemianæsthesia rather than with general insensibility, and there is the important fact that it occurred in a man rather than in a hysterical girl, and probably as a result of some slight organic lesion. The actual inability to move when the eyes were closed was here, moreover, limited to the lower extremity; though the power of moving the upper extremity under the same conditions was much more than usually affected. This case is in marked contrast with the ordinary run of cases of hemianæsthesia, in which there is generally no very appreciable loss of "muscular sense."

"W. P.——, aged 43, married, a plumber and gas fitter, was admitted into the Westminster Hospital, under Dr. Fincham's care, on April 25, 1865. He is tall, fairly nourished, with a sallow complexion, and there is a faint blue line along the edge of his gums. He has never had gout, rheumatic fever, or syphilis. A year ago he had an attack of lead colic, followed by dropping of the left wrist, which only lasted a few days.

"*Present state:*—There is complete *analgesia* of the whole left half of the body, exactly limited to the median line—namely, of the left half of the head, face, tongue, palate, neck, trunk, and penis, and the left arm and leg. Pricking and pinching, except in the spots to be presently mentioned, are obscurely felt as a mere contact, and many seconds after the impression is made. The left eyeball is so insensible to pain that the patient rubs it with impunity. The left half of the tongue cannot distinguish sapid substances, and the patient complains of a sensation of heat and dryness, and occasionally of pins and needles in that side of the organ. Instead of analgesia, there is tenderness on pressure in front of the left elbow-joint, and again near the lower edge of the deltoid. Differences of temperature are more acutely perceived on the affected rather than on the healthy side. There is nearly complete *anæsthesia* of the left half of the body, for when any point is touched on that side, the patient becomes conscious of it, only after an interval of several seconds, and besides, localizes the impression erroneously. He states that he feels as if through a thick layer of flannel or wool. He does not know the position of his left arm or leg, if he does not see it. *If, when his eyes are shut, he be asked to touch the tip of his nose with his left hand, he visibly makes considerable efforts, but his hand, which is raised with great difficulty and slowness,*

[1] Translation of Trousseau's 'Lectures,' 1866, p. 213.

is extremely rare. He, however, gives the details of another case,[1] just as extraordinary as that recorded by Duchenne, this patient having been under his care for nearly twelve months in 'la Charité.' Here are a few quotations extracted from his report:—

The patient was a girl, 21 years of age, suffering among other things from complete loss of taste, from noises in the ears, and from amblyopia especially on the left side.

"The skin of the whole periphery of the body is completely insensible; the patient absolutely does not feel anything which she touches.

"The muscles of the limbs do not feel pressure; they require to be pressed strongly against the bones before the patient feels anything. Passive movements are not at all felt, so that the patient has no consciousness of them if sight does not intervene.

"In short, the anæsthesia of the surface of the body is so extreme that, having bandaged her eyes, the patient has been taken from her bed with only her night-dress on, placed upon the floor of the ward where she has been left a few moments, and then replaced in her bed, without her having any consciousness of what had been done. The patient describes her condition by saying that she is like a balloon suspended in the air. She has only the sense of sight, which is weak, and that of hearing, which is just as bad.

"The muscles of the upper extremities are notably weak, nevertheless the patient can sew pretty well; she moves her fingers and hands well, but *if one prevents the intervention of sight no movement is any longer possible: whatever may be the efforts of the will, there is complete immobility.*

"The lower extremities are still more feeble; the patient can, whilst in bed, execute some movements of the whole limb, but she cannot move the toes, and when she is placed on her legs they immediately double under her, and there would inevitably be a fall [if she were not supported]."

After she had been in the hospital about four months this patient was in some respects better, she could for instance maintain herself in the standing position when supported on both sides, but as regards her knowledge of passive movements, and of the position of the limbs, there was no improvement whatever.

Fortunately I have been able to find the record of another

[1] 'Traité de l'Hystérie,' Paris, 1859, p. 304.

either stops at a certain height, or goes to one side of the head, never succeeding in touching the nose. The movement is slow and hesitating, never abrupt or jerked. *When his eyes are shut, he cannot make the least movement with his left leg.* When he walks, he keeps his eyes fixed on the ground immediately before him, not on his legs. His gait is somewhat uncertain and unsteady, but not markedly so. *He cannot walk with his eyes closed;* he cannot stand, even with his eyes open, when his feet are closely approximated, and he looks straight before him. *If, while his feet are wide apart, he shuts his eyes, he immediately oscillates from before backwards, and threatens to fall down.* He says that he then feels as if he had only one leg.

"There is no real *diminution of motor power* at present, at least beyond a slight depression of the left angle of the mouth, and a scarcely perceptible deviation of the apex of the tongue to the left side. There is no difference of size between the muscles of the right and left limbs; electro-muscular contractility is perfect on both sides, but electro-muscular sensibility is almost *nil* on the left. None of the senses are affected, except *taste* in the left half of the tongue. Vision is good; the left pupil is, however, appreciably smaller than the right. Hearing is perfect on both sides. The intellect was never affected; memory is very good, and the man gives an excellent account of himself. There has never been any headache, but more or less giddiness throughout. Articulation thick and embarrassed, probably owing to the numbness of the left half of the tongue and soft palate; but the faculty of language is unimpaired. Appetite good; digestion easy; bowels at present regular, but were formerly very costive. The bladder was never affected.

"*Mode of Attack.*—The patient's illness dates from the beginning of September, 1864. It set in suddenly, without any premonitory symptoms, about seven o'clock one morning, with a sense of chilliness and numbness all down the left half of the body. He was not, however, prevented from attending to his usual occupation, until three weeks afterwards, when the numbness was replaced by complete insensibility. There was also some motor paralysis at first, as it appears from his statement; for he affirms that there was real and considerable weakness of his left arm and leg, that his left cheek was pendulous, and that the right angle of his mouth was pulled upwards and outwards whenever he spoke or laughed. Until he was examined by Dr. Fincham, he had not discovered his inability to move his left arm and leg unless he looked at them. Under the influence of faradization, and the administration of iodide of potassium for a short time, and afterwards of hypophosphite of soda, he gradually improved."

So far the cases to which I have referred have belonged to one or other of two categories, that is they have been either (*a*) cases of locomotor ataxy, or (*b*) they have been cases in which there has been loss of sensibility, deep as well as superficial, either unilateral or general. It is, indeed, now well known that these are the two forms of disease in which we are entitled to look very frequently for some loss or defect in kinæsthetic impressions generally, and of those pertaining to the "muscular sense" in particular. It is, however, also well known that all kinds of kinæsthetic impressions are by no means equally or necessarily affected in these two diseases. In each of them, for instance, ordinary cutaneous sensibility, and possibly even deep sensibility, may be gravely impaired or lost, when there is comparatively little loss of special "muscular sense" impressions.

Thus in locomotor ataxy it is now well-known that there is no regular parallelism between the loss of ordinary modes of sensibility, and the loss of special "muscular sense" impressions. In some rare cases of this disease there may be very little loss of ordinary modes of sensibility, and yet a marked deprivation of "muscular sense" impressions. The first case recorded in Landry's memoir seems to have been, in its early stages a well-marked one of this kind, and Trousseau said that out of about fifty cases of locomotor ataxy he had seen three of this latter type. This is a class of cases, however, about which we want further precise information.

Similarly, in regard to hemianæsthesia, it is now well known to be quite exceptional to meet with such cases as those recorded by Demeaux (p. 11), in which there was complete loss of special "muscular sense" impressions. The rule has been, for instance, with the hemianæsthetic patients which have been so thoroughly investigated by Charcot at the Salpêtrière that although there has been complete loss of tactile sensibility, and usually absolute insensibility to pain in the skin and all other sensitive structures on the affected side, together with slight paresis of the affected limbs, the so-called "muscular sense" has been nearly always preserved. Their ability to perform even complex muscular movements when the eyes are closed has remained quite intact. This

was well seen, for instance, in the case of one of his hemian-æsthetic patients who was hypnotised by Charcot in the presence of a party of distinguished observers when the following phenomena were witnessed.[1]

"At 10.7, the patient being asleep, Professor Charcot told her to rise and take a chair. She did so, her eyes being closed and her eyelids tremulous. Having seated herself he told her to write her name. Pen, ink, and paper being furnished her, she sleepily wrote her own name, and afterwards, when ordered, Professor Charcot's, *her eyes remaining closed the whole time.* Whilst writing the skin over the right wrist was transfixed by a thick needle, but the patient appeared quite unconscious of the operation, continuing to write with the needle *in situ.* It is to be remarked that during the mesmeric state, the patient is anæsthetic on both sides of the body; whilst, in the waking state, it is only the left side which is anæsthetic. At 10.10 Professor Charcot told the patient to begin to sew. A half-hemmed towel was handed to her, and she at once commenced to sew with great dexterity and rapidity, co-ordinating admirably the movements of the two hands. The patient was then told to rise and go into an adjoining room used for photography; she did so, and on her way, having to descend a step, she walked with caution and safety, *her eyes being still closed.*"

Phenomena of the same kind, as well as others, were shown to Prof. Rouget of Montpellier and myself on the following day by Prof. Charcot, demonstrating that even in well-marked cases of superficial and deep anæsthesia, there may be no loss of the muscular sense or difficulty in executing even complicated voluntary movements when the eyes are closed. In a small minority of cases of hemianæsthesia, however, it is not so, and then we have to do with such phenomena as were met with in the typical case recorded by Demeaux if not such extreme results as those met with in the cases of Duchenne, Briquet and Bazire. We stand much in need of further records concerning this small minority of cases.[2]

[1] As recorded by Dr. Arthur Gamgee in 'Brit. Med. Journ.,' Oct. 12, 1878, p. 545.

[2] Such facts have been strangely lost sight of by Prof. Ferrier when seeking to interpret the results of his experiments upon the cerebral cortex of monkeys. It is difficult to understand, for instance, how he can make such statements as these ('Functions of the Brain,' 2nd ed., p. 61):—"There are certain clinical

Other cases, however, in which there is a marked defect in "muscular sense" impressions, appear to exist which cannot be included in either of the previous clinical categories. We must regard them as constituting a class apart, (*c*) the nature and clinical character of which have hitherto been extremely ill-defined. Landry recorded, but unfortunately in abstract only, two such cases. Brief though the details are, it will be well, however, to reproduce them here, as they are cases in which tactile sensibility was either unimpaired or but slightly affected.[1] The nature of the first case, more especially, is very obscure, owing to insufficiency of clinical details.

"Obs. IV.—The woman Reine, 42 years old, a seamstress, of a nervous lymphatic temperament, but strongly built, entered on March 20, 1852, the Beaujon Hospital (under the care of M. Sandras). After having experienced, in the course of the year 1849, headaches accompanied by giddiness, subjective noises, dizziness, and loss of appetite, she began to experience tinglings with a sensation of numbness in the upper and lower extremities;

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facts which would seem to show that the sensibility of the muscles, and the muscular sense may continue though cutaneous sensibility is abolished. But, though the possibility of this cannot be denied, the actual existence of such a condition is far from being satisfactorily proved. Much of the evidence adduced is extremely unsatisfactory, and there is reason to believe that tactile sensibility is not entirely abolished, though there may be insensibility to pain and temperature, in the cases where the muscular sense really continues." Further on, after quoting some details concerning the rare and exceptional case of hemianæsthesia recorded by Demeaux, he says (p. 325), "These facts show clearly that the same condition which abolishes cutaneous sensibility, also entirely annihilates the so-called muscular sense," adding, "from such and similar cases, many of which I have myself seen and investigated, and multitudes of which are to be found in medical literature," etc. I should be extremely glad if Prof. Ferrier would give us some references to other cases like those of Demeaux, "multitudes" of which, as he says above, are to be found in medical literature. What he writes also on p. 344 (*loc. cit.*), seems to show that he considers it to be the rule that "muscular sense" is lost in cases of cerebral hemianæsthesia.

[1] These two cases, as well as those which follow (recorded by Zenner and Horsley) ought to prove interesting to Dr. Ferrier, considering the very positive views which he has recently expressed. Thus, he says ('Functions of the Brain,' 2nd ed., 1886, p. 380):—"There is no necessary connection between the power of directing movements and the muscular sense, as has been erroneously assumed by Brown Séquard and others. Loss of the muscular sense never occurs without general anæsthesia of the limb. No one has even furnished the slightest evidence of impairment, or loss of the muscular sense apart from profound impairment or total abolition of the common sensibility of the limb." Each of these statements is, in my opinion, thoroughly inaccurate.

at the same time her sight failed a little. In 1850 the lower extremities appeared to lose power, and her gait became staggering. From that time a paralytic affection developed which made continual progress.

"On her entry into the hospital (March 20, 1852), walking could not be accomplished without the aid of a stick. There was great difficulty in rising and in preserving her equilibrium; irregularity in the movements of the lower limbs; a tendency to assume an accelerating gait; power of executing all movements, which were extensive and energetic: *unconsciousness of passive movement; imperfect appreciation of the position of the lower extremities and of the extent of their active movements; defective measuring of movements when the eye does not watch them*, etc. A slight feebleness of the hands. *Sensibility absolutely intact.* Micturition and defecation normal. Integrity of intelligence and of all the senses excepting sight which is much enfeebled. No local morbid sign on the side of the nervous centres. . . . In February, 1853, there was an aggravation of all the symptoms. No relief had been derived from various kinds of treatment."

There is also no sufficient clue as to the nature of the next case. It occurred in a hysterical patient; though it seems very possible that it may have been due to a tumour in the right Rolandic area.

"Obs. V.—Angélique M. . . ., 46 years of age, a worker in furs, strongly built, but actually in a profoundly cachectic condition, of a nervous lymphatic temperament, entered, on Dec. 9, 1851, the Beaujon Hospital under the care of M. Sandras.

"This patient, subject to hysterical attacks for nearly twenty years, perceived at the end of 1849 that the left hand was becoming weak and numb. A paralysis of the whole limb soon developed, characterized by these symptoms: execution easy, though feeble, of all movements of the fingers, of the hand, and of the forearm, but without precision or being properly measured; *defective appreciation of passive movements and of the extent as well as of the energy of active movements; impossibility of appreciating weight.*

"All the functions of the upper limb are profoundly altered, and the patient cannot put it to any use which demands a little delicacy in the movement.

"*Sensations of contact and of temperature preserved;* sensations to pain obtuse and abolished in places. Sight very feeble; all the other senses are intact. No morbid local sign on the side of the nervous centres. Chloro-anæmic cachexia. Hysterical phenomena."

Another of these cases has, however, been recently recorded by P. Zenner[1] in which there is very strong reason for believing that the symptoms were produced by injury to Ferrier's so-called "motor centres" for the arm. The evidence in favour of this localisation is altogether similar in nature to that upon which Ferrier has himself relied, in several cases, in his lectures on the "Localisation of Cerebral Disease."

"A. N., age 23, shoemaker, was wounded during the court-house riot, March 29, 1884. The wound was on the right side of the head and laid bare the bone, on the surface of which a linear fracture, without any depression, was seen. It was made by a ball which had ploughed through the scalp and just grazed the bone. The wound was four inches long, and about two and a half inches from the median line, and would have been equally divided by a line passing from one auditory meatus over the vertex to the other.

"The man was unconscious for some hours after the injury, and there was occasional delirium for a period of two weeks. When consciousness returned it was observed that the left arm was entirely powerless, and that the face was drawn to the right side. No observation of the condition of the lower extremities was made. Shortly after the injury there was loss of sensibility in the hand; a pinch was not felt. From the second or third day, for about a week, there were frequent convulsive movements of short duration, apparently limited to the muscles of the face and unattended by loss of consciousness.

"Within a week power of movement at shoulder and elbow had returned, though the hand and wrist were yet powerless. After a few weeks a paresis of the left hand alone remained.

"I first saw the patient seven months after the date of the injury. The foregoing statements were obtained partly from the family, partly from Dr. N. P. Dandridge, who saw the patient about a week after the wound had been received. I saw the patient three times the latter part of October, and December the 1st, 1884, and May 16, 1885. As my first examination was a hasty one and no notes were taken, I will give a description from notes of my examination on December 1st.

"He is a man of average size, well built, and of good muscular development. On the right side of the head there is a linear cicatrix two and a half inches in length. The direction of the cicatrix is parallel with the sagittal suture, from which it is

[1] 'Journal of Nervous and Mental Disease,' New York, 1886, p. 438.

distant about two and three quarter inches. The central part of the cicatrix, in an antero-posterior direction, is just above the meatus auditorius. Patient does not suffer with headache, or other subjective symptoms, and, in his own opinion and that of his family, his mental powers are unimpaired. There is no impairment of sensation or motion anywhere except in the left upper extremity. He can walk long distances without difficulty, and the face, both in repose and action, shows no motor defect."

"*Left upper extremity.*—Power in the movements at the shoulder, and extension of the elbow about equals that of the right side. Cannot flex the elbow quite as forcibly as the right, but, perhaps, the difference is not greater than could be accounted for by its being the left arm, and its having been used less than the other for some time. Grasp in left hand fair, but quite appreciably less than in the right. But the difference in the power of the flexors of the fingers is more apparent when he attempts to flex one or several fingers separately. Then there is a decided difference in the two hands, the greatest difference being observed in the middle and ring fingers. This difference is also manifested when he holds an object, as a pencil, between his finger-tips. He holds it well with the left hand, but with decidedly less force than with the right.

"Cutaneous sensibility is good everywhere, excepting over the fingers. Elsewhere he feels the lightest touch (though in the left palm it is less accurately localized than in the right), but in the last two phalanges, both in their dorsal and palmar surfaces, he only becomes aware that the parts are touched when decided pressure is made. A small piece of coin, whose exact character is immediately recognized by the right hand, is often not felt at all when placed between the fingers of the left hand.

"*The knowledge of the position of the fingers is impaired.*—This is most marked in the middle and ring fingers. Occasionally he does not seem to know (eyes being closed) that the finger is moved. At other times he mistakes which finger it is. On the right side his knowledge in this respect is perfect.

"Actions requiring some delicacy of movement are performed very awkwardly. He buttons his coat with the left hand with the greatest difficulty.

"The only deep reflex action which could be elicited was a slight movement in tapping the lower end of the radius. The same reflex could not be elicited in the other side.

"A word more as to the improvement of the patient. According to the statement of the family, there was continuous improvement for six months, after which his condition appeared to remain

stationary. I first saw him seven months after he sustained his injury. As before stated, my examination at that time was hurried, and no notes were taken; but my impression is that both the impairment of sensation and muscular weakness were greater than they were at the time of my second examination, some five weeks later. But his condition May 16th (nearly fourteen months after the injury) was in no wise different from what it was December 1, 1884, unless it be that delicate movements, as buttoning his coat, were performed with more ease."

In this case there is strong reason for believing, as Zenner points out, that we have to do with a limited lesion of the cortex in the Rolandic area. The loss of "muscular sense" as well as the defect of tactile sensibility from a lesion so situated would, of course, be a matter of extreme importance. But the evidence in favour of such impairment of the "muscular sense" in particular, as a result of cortical lesions in the Rolandic area, is greatly strengthened by the following cases, extremely important in more ways than one, which have been recorded by Victor Horsley in a communication made by him to the recent meeting, in August last, of the British Medical Association.[1]

Case I.—James B., aged 22, was admitted into the National Hospital for Paralysis and Epilepsy, under the care of Dr. H. Jackson and Dr. Ferrier.

Past History.[2]—At the age of 7, the patient was run over by a cab, in Edinburgh. He was at once admitted into the Royal Infirmary, under Professor Annandale, who found a depressed comminuted fracture, with loss of brain-substance, in the situation mentioned below. The fragments of bone, etc., were removed, and the wound ultimately healed, although it suppurated freely, and hernia cerebri occurred. The patient was hemiplegic for some time, but gradually (seven weeks) the paralysis disappeared. At about 15 years, the patient began having fits, which were very intermittent. He was admitted into the hospital in 1885, when he had an enormous number of fits, and for some days was in the *status epilepticus*. After discharge from the hospital, he had

[1] *See* 'Brit. Med. Journal,' Oct. 9, 1886.

[2] Owing to the great courtesy and kindness of Prof. Annandale, who supplied Dr. Ferrier with an elaborate description of the case while under his care, I am fortunately able to furnish an accurate history of the injury causing the epilepsy. (V. H.)

no fit for seven weeks, at the end of which period they returned, and for three days before admission he was again in the *status epilepticus*.

Present state.[1]—On the left side of the vertex of the head (the exact site, as determined by measurement, being the centre of the upper third of the ascending frontal convolution; that is, posterior to the hinder end of the superior frontal sulcus) there was a quadradiate scar, opposite the centre of which the bone could be felt to be wanting, so as to form an oval opening in the skull, the long diameter of which was about one inch, and parallel to the sagittal suture. Pressure on this scar always gave pain, which was very greatly increased when the patient was suffering one of his paroxysms of fits.

Fits.—The fits, which occurred in batches (at this time the patient had 3,000 in a fortnight), were almost always of the same character, usually commencing in the right lower limb, sometimes in both the right limbs simultaneously. An example of a fit of the first category is as follows:—

"The right lower limb was tonically extended, and the seat of clonic spasm. The right upper limb was then slowly extended at right angles, to the body, the wrist and fingers being flexed; the fingers next became extended, and clonic spasms of flexion and extension affected the whole limb, the elbow being gradually flexed. By this time, spasms in the lower limb having ceased, but those in the upper limb continuing vigorously, spasm gradually affected the right angle of the mouth, spreading over the right side of the face, and followed by turning of the head and eyes to the right."

To sum up, the parts affected were so in the order of lower limb, upper limb, face, and neck; the character of the movements was, first, extension, then confusion, finally, flexion,[2] showing clearly that the focus of discharge was situated around the posterior end of the superior frontal sulcus, this point coinciding, as mentioned above, with that found by actual measurement. Before going on to describe the surgical treatment, it is important to mention that the patient was distinctly hemiplegic, even ten days after the last fit, but he could perform all the movements of the right limbs, though about half as strongly as on the left side, there was no affection of sensation on the right side, while the reflexes, superficial and deep, were exaggerated in both the right limbs.

[1] Only those facts are given which directly bear on the cerebral disturbance.

[2] See a paper by Dr. Beevor and myself, to be shortly published by the Royal Society in the 'Philosophical Transactions.' (V. H.)

Operation, May 25th, 1886.—According to the method described in the foregoing paper, the bone around the old opening was freely removed, the dura mater, arachnoid, and skin being found to form a homogeneous mass of fibrous tissue; the former was raised with the flap. The scar in the brain was found to be highly vascular, of a deep red colour, and about three centimètres long and two broad. The membrane covering the brain around appeared to be very opaque, and the brain of a slightly yellower tinge than usual. The scar, and about half a centimètre of surrounding brain-substance, was excised to the depth of two centimètres. It was then found that the scar-tissue penetrated a few millimètres further into the corona radiata fibres of the marginal convolution. This portion was then removed, and the wound closed. In the removal of the mass, three fair-sized veins, coming directly from the middle of the area for the upper limb, had to be ligatured, since they passed directly into the scar. The wound completely healed in a week. The tension of serum was twice relieved (once, probably unnecessarily). The most interesting point now to be recorded is, that after the operation the patient was at first completely paralysed in digits of the right upper limb; and for further flexion of the wrist and supination of the fore-arm. Coupled with this motor paralysis, there was loss of tactile sensibility over the dorsum of the two distant phalanges of the fingers. He could not localise the touch anywhere below the wrist within the distance of one internode; finally, he could not tell the position of any of the joints of the digits. Then we have here, apparently, a distinct instance of loss of tactile sensibility and muscular sense, coupled with motor paralysis, all due to lesion of the cortex[1]. It cannot, however, be too clearly understood that it is very possible that some of the fibres coming from the gyrus fornicatus in the corona radiata may very probably have been injured. This condition of motor and sensory paralysis gradually disappeared in the course of the next two months. Up to the present time the patient has had no fits.

Case II.—Thomas W., aged 20, was admitted into the National Hospital under the care of Dr. Hughlings Jackson.

Family History.—There was nothing important, save that a paternal aunt died of consumption.

Past History.—He had had many attacks of pleurisy after the age of 15.

History of Present Illness.—He began, in January, 1884, to have "cramps" in the left thumb and forefinger, these consisting of

[1] By this I mean, of course, the disturbance in the area for the upper limb produced by the ligature of the veins coming from it, as noted above. (V. H.)

clonic opposition of the named digits, and occurring about twice a day for three months. The first severe fit occurred in March, 1884. Spasm spread up the arm, and the patient fell. He had the second in January, 1885. Then followed a series of remissions of the twitchings, until, in August, 1885, another severe fit commenced a series of fits, occurring once or twice a week, until admission on December 4th, 1885. The character of the fits was almost always the same. They began by clonic spasmodic opposition of the thumb and forefinger (left), the wrist next, and then the elbow and shoulder were flexed clonically, then the face twitched, and the patient lost consciousness. The hand and eyes then turned to the left, and the left lower limb was drawn up. The right lower limb was next attacked, and, finally, the right upper limbs. Paralysis of the left upper limb frequently followed a fit. At frequent intervals every day the patient's thumb would commence twitching, but progress of the convulsion could often be arrested by stretching the thumb or applying a ligature. In February and March, 1886, the twitchings frequently commenced in the face, but in April again the thumb was the most frequent seat of origin of the fits.

Present State (much abbreviated). *Motion.*—The grasp of the left hand was 45; of the right, 85. He could perform all movements with the left upper limb, though those of the hand were rather enfeebled. The left thumb was frequently in a state of rigidity, alternating with clonic spasm. (This state could easily be induced by manipulating the thumb.) *Sensation.*—There was no affection, save loss of muscular sense (that is, sense of position, etc.) in the left thumb. *Deep Reflexes* were exaggerated in the left upper limb. The patient frequently had severe headache, beginning at the occiput, and shooting forward, especially to the right parietal region. The *Optic Discs*, examined by Mr. Marcus Gunn, appeared to be normal, though very pink (physiological hyperæmia).

Diagnosis.—In a paper referred to in the foregoing case (No. I.), Dr. Beevor and myself have shown, that the movement of opposition of the thumb and finger can be elicited by minimal stimulation of the ascending frontal and parietal convolutions at the line of junction of their lower and middle thirds. Dr. Hughlings Jackson witnessed one of our experiments demonstrating this fact, and expressed his belief that this patient (Case II.) was suffering from an irritative lesion of unknown nature, situated in the part of the brain thus indicated. Confirmation of this opinion was to hand in the order of march of the spasm, which was in nature and arrangement in exact accord with

the results of the investigations referred to. An exploratory operation was therefore decided upon.

Operation (June 22nd, 1886).—The seat of the lesion having been determined by measurement, the large trephine was applied; and, on raising the dura mater, a tumour came into view. More bone was removed above and in front, so as to completely expose the mass to which the dura mater was adherent. The border of the tumour stood out about one-eighth of an inch from the surface of the brain, and it was much denser than the brain-substance. It appeared to be only half an inch broad, but as the brain-substance all round it for more than half an inch appeared dusky and rather livid, I removed freely all the part apparently diseased. (As is shown in the photograph and specimen, this procedure was fully justified, since the growth spread very widely under the cortex.) Before closing the wound, the centre of the thumb-area was removed by free incision. This detail Dr. Jackson and myself had resolved to carry out in the possible event of there being no obvious gross organic disease, in order to prevent, as far as possible, occurrence of the epilepsy.[1] Numerous vessels were ligatured, especially three or four at the upper border of the growth proceeding from the rest of the cortex, for the movements of the upper limb. The wound was closed as before. Five-sixths of it healed by the first intention in a week, in spite of the fact that there was considerable œdema of the scalp, due to irritation by the carbolic gauze (removed speedily by changing to eucalyptus gauze). The remaining sixth, just at the lower border of the flap, gave way, and healed by granulation, after separation of a small piece of skin at the edge. The after-condition of the patient was most interesting and important. There was, next day, partial motor paralysis of the left side of the face (lower division), complete motor paralysis of the left upper limb, from and including the shoulder. On the 27th June, there was noted left hemianæsthesia to a light touch (sensibility to pain unaltered), localisation of a prick of a pin very deficient all over the left side, perfect on the right, complete loss of muscular sense in the left upper limb below the shoulder. On the second day after the operation, when making an effort to move the left upper limb, the patient suddenly put his hand to the wound, and said he felt a

[1] To discuss this point in detail would entail excessive lengthening of this paper; but I wish to point out that, as strongly urged by Dr. Jackson, the removal of an epileptogenous focus is not only justifiable, but called for. The exact localisation could be ascertained by the use of the induction current, the employment of which means I resorted to for diagnosis nearly three years ago. 'Brain,' Vol. VII., p. 232. (V. H.)

"buzzing" in the head there. When the left upper limb was passively moved, he also complained that pain seemed to shoot up the "bones of the limb," side of the neck, and through the hand to the wound. The deep reflexes were much exaggerated on the left side in both limbs. All the above conditions gradually improved, and at the date of the meeting, the patient had regained everything, except that the grasp of the left hand was not quite so good as before, and the five movements of the fingers remained hampered. Further. the deep reflexes on the left side are at the present time still very much exaggerated. The patient had, in July, a few slight twitches in the three right fingers; none in the thumb or index; no fits since operation. The tumour was composed of dense fibrous tissue, with two caseated foci, microscopical examination proving it to be tubercular.

It is unfortunate that in the first of these cases no observations were made upon the state of the lower extremity in regard to its "muscular sense" impressions. Such cases require to be examined with the greatest care in order to obtain trustworthy information concerning the "muscular sense." We must look therefore for further information in these directions, and no cases will be so important as those in which portions of the Rolandic area have been actually excised by the surgeon. They are veritable experiments, from one point of view, calculated to throw light in directions from which none is to be hoped for from experiments upon animals not endowed with the gift of speech, in regard to the presence or absence of "muscular sense" impressions, and, it should be added, even of tactile impressions in the hand in cases, for instance, where corresponding portions of the Rolandic area have been removed by operation.

A very important paper by Westphal in reference to some of these questions, is to be found in the "Charité-Annalen" [1] concerning a case of wasting disease in the ascending parietal convolution and parietal lobule (though not absolutely limited thereto) in which, in addition to other troubles, there was the association of paralysis with marked loss of "muscular sense," and some diminution in tactile sensibility.

It seems best to defer, for the present, any further remarks

[1] VII. Jahrg., 1882.

as to the paths or cortical termini for "muscular sense" impressions.

Meanwhile, what has already been brought forward will show that three kinds of defects related to one another, it is true, are to be looked for in cases where special "muscular sense" impressions are either absent or defective. These defects are as follows:—

(1) Defective knowledge of the extent of movements executed, either actively or passively, as well as of the position of the limbs, when the eyes are closed.

(2) Difficulty in discriminating differences in weight or degrees of resistance when muscles are called into play.

(3) Difficulty in accurately performing given movements when the eyes are closed.

It is, of course, of some importance to know whether these three kinds of disability are referable to loss of kinæsthetic impressions generally or whether they are specially related to loss of the most important group of these kinæsthetic impressions, viz., those of the "muscular sense."

This subject has already been alluded to in part; it has been stated for instance, that in the majority of hemi-anæsthetic cases, there is (3) no difficulty in accurately performing movements when the eyes are closed. This is a matter of common observation, but it has been thoroughly established of late years by Charcot and others.

Again, it has been shown more especially by the observations of Leyden [1] and Bernhardt,[2] that the sensibility of the skin may be partially or completely destroyed without appreciably altering (2) the patient's ability to discriminate differences in weights when his muscles are called into play. It may be stated that in applying this test to the upper extremities, leather balls of the same size but differently weighted within with lead may be used, or where these are not to be had we may roughly test the ability to discriminate differences in weight (the patient's eyes being closed) by placing piles of pence, or books of the same size except as regards thickness, upon the extended palm. In the examination

[1] 'Archiv de Virchow,' t. 47, p. 325, *et seq.*

[2] 'Archiv für Psychiatrie,' iii., pp. 618 and 632.

of the lower extremities different weights may be suspended from the extended limbs. On this subject Bazire [1] has said:—

"Quite recently Dr. Jaccoud has applied Weber's method to the lower extremities, and the results of his experiments, made on twenty-four different individuals, tend to show that it is possible, in health, to distinguish on an average a difference of from 50 to 70 grammes (say about two ounces) between two weights successively suspended from a lower limb. In six ataxic patients to whom this test was applied, the minimum of the difference in weight appreciated by the patient was found by Dr. Jaccoud to be considerably above the healthy average, varying from 100 to 3000 grammes (from 3¼ ounces to four pounds)."

Spaeth [2] also, without being aware of the experiments just cited, adopted a similar method to investigate the state of the muscular sense in two cases of ataxy, and in both he found a notable diminution in the individual's discriminative power—indeed, in one of the two cases no difference was perceived unless the successive weights bore the ratio to one another of 1 to 100.

There can be no doubt that some of a patient's ability (3) to discriminate the extent of active or passive movements executed, as well as some of his knowledge of the position of his limbs, depends upon kinæsthetic impressions coming from the skin and from joints. Some have supposed that all this kind of knowledge comes from these ordinary kinæsthetic impressions.

Wundt, for instance, thinks that these sensations belong to a different category from those by which we appreciate weight or resistance.[3] Gley [4] also seems to think that the knowledge of the position and movements of the limb is derived in the main, if not wholly, from skin and joint impressions.

This, however, is in my opinion an erroneous conclusion, based upon the examination of one of the exceptional cases of hemianæsthesia, like that recorded by Demeaux (p. 11). A study of the two cases last quoted from Landry [5] shows conclu-

[1] Translation of Trousseau's 'Lectures,' 1866, p. 212.
[2] Beitr. zur Lehre von den Tabes Dorsalis. Tübingen, 1864.
[3] *Loc. cit.*, t. i., p. 426.
[4] Rev. philosoph., December, 1885, p. 605.
[5] Obs. iv. and v. (see pp. 23 and 24).

sively that knowledge as to the position and movements of the limbs may be more or less lost even when cutaneous sensibility is not at all or but little impaired. Other good clinical evidence may also be adduced pointing in the same direction. Thus Bazire, in reporting a case of locomotor ataxy, says[1] :—"His gait was very characteristic of locomotor ataxy. . . Within the last few weeks he had noticed that he could not tell the position of his limbs in bed, under the bed-clothes. He still complained of numbness of the legs, and yet, strangely enough, when tactile sensibility was tested with the æsthesiometer, it was found unimpaired, for he could feel the two points of the instrument at a distance of only two inches, when applied in front of the legs." In cases of this kind, it could only be expected that there would be a defective knowledge of the position of the limbs while the limbs are at rest, in bed. As soon as they are moved, when sensibility is either intact or but little impaired, the accompanying superficial sensations must necessarily give a pretty accurate knowledge as to their position. This source of difficulty is present, therefore, in all cases in which superficial insensibility does not exist though loss of the muscular sense is suspected; we cannot well move the limb or parts of the limb (such as the fingers) about at random, with the view of asking in what exact position they are ultimately left, without giving the patient *some* aid through his intact sense of touch. Of course if, in such a case, we still find that the patient has a defective appreciation of the position in which his limbs are ultimately left, the evidence is all the stronger that his "muscular sense" must be defective.

It thus seems clear that of the three disabilities above named, the second and third are dependent almost if not entirely upon loss of the muscular sense, whilst the first (the knowledge of the extent and direction of passive movements, and of the position of limbs) is very greatly due also to "muscular sense" impressions, though it is in part undoubtedly derived from other less special kinæsthetic impressions.

In regard to the very extraordinary cases recorded by Duchenne, Briquet, and Bazire, in which the third of the above-named disabilities was carried to such an extent that

[1] Trousseau's 'Lectures,' translated by Bazire, part i., case vi., p. 195.

even the simplest movements could not be performed at all without the aid of the sense of sight, it seems to me that we have here to do with functional defects in the cortical termini for "muscular sense" impressions, as well as interference with the functional integrity of the afferent channels for such impressions. These latter are alone affected (in concert with the afferent channels for other sense impressions) in those more ordinary severe cases of hemianæsthesia of that type recorded by Demaux. But in these altogether extraordinary cases of Duchenne and Briquet there existed, I think, in addition a low functional activity of the "muscular sense" centres[1]; that is to say, these centres could not be roused into activity by comparatively weak associational stimuli (as during volition with eyes closed); though they were able to respond under the influence of a stronger stimulus from the visual centre. May we not have in certain *hysterical paralyses of cerebral type* a still greater functional degradation of these "muscular sense" centres? Such patients do not move the affected limb or limbs either when their eyes are open or closed.

We may find illustrative, though not exactly parallel, conditions among speech defects of an amnesic type, due to a lowered functional condition of the auditory centre. When these, for instance, cannot be roused into proper activity by ordinary associational stimuli, the patient, as a consequence, cannot recall the words that he wishes; and, as a further consequence, cannot utter such words. This is a kind of paralysis, so far as these speech movements are concerned. On the other hand, such a patient may be able to repeat at once words uttered before him by another person. The affected auditory centre in this case reacts to the stronger stimulus; just as I imagine the affected "muscular sense" centres reacted in the cases of Duchenne, Briquet, and Bazire, under strong associational stimuli coming from the visual centres.

Supposing, however, the left auditory centre to be more damaged still, either functionally or structurally, speech becomes impossible. Speech is paralysed, in fact, in such a case, if the degradation of the auditory centre be of functional type, in just the same way as I suggest a hysterical patient is

[1] See 'Brain as an Organ of Mind,' pp. 616 and 621.

paralysed when she is suffering from a very low functional condition of her "muscular sense" centres for leg or arm, or both.

It is extremely interesting to find, in the case of Bazire, the complete inability to move the limb without the stimulus of sight impressions, existing only in the leg, while in the arm there was an approximation to this condition.

A fuller understanding of my views, in regard to these very interesting cases, may be expected after the reader has studied a subsequent section of this paper on the part taken by "Muscular Sense Impressions in the Execution of Voluntary and Automatic Movements" (p. 52).

Having said thus much concerning kinæsthetic impressions as a whole, and "muscular sense" impressions in particular, it may now be well to endeavour to show that both kinæsthetic impressions as a whole and these "muscular sense" impressions in particular, are, like other sensations, capable of being revived in idea. It seems desirable to advance something like a formal proof of this here, as, in our opinion, these ideal revivalsof kinæsthetic impressions aid in the performance of most important functions—that is, in the execution of all the voluntary movements we perform.

Just as the blind man can recall to mind previous sight impressions, or the deaf man sounds previously heard, so can the man who has had an arm amputated call to mind movements which this limb had formerly executed, by reviving in idea the kinæsthetic impressions previously associated therewith. Many remarkable instances of this have been recorded by Weir-Mitchell in his work entitled 'Injuries of Nerves and their Consequences' (p. 348, *et seq.*). I am inclined to think that in very many of these cases the conjuring up of subjective impressions of the several kinds mentioned is more facile than in persons who are neither blind, deaf, nor armless; and that, in many instances, the impressions so revived, assume such an intensity as to appear more like hallucinations than ordinary sensory revivals. Weir-Mitchell says:—"Nearly every man who loses a limb carries about with him a constant or inconstant phantom of the missing member, a sensory ghost of that much of himself. . . . The sensation of the presence of

the part removed exists in many persons as soon as they come from under the influence of the anæsthetic used at the time of the amputation, but in others it only arises after they cease to suffer pain, being rarely delayed beyond three weeks. The more healthy the stump, the less perfect after a time becomes the sense of the existence of the limb removed, while it is liable to be recalled by a blow or anything which causes a return of subjective sensation. . . . Even in those who are least conscious of the missing part, I have amazed them by suddenly recalling it with the aid of a faradic current applied to the nerves of the stump. It is not easy to forget the astonishment with which some of these persons re-awaken to a perception of the long lost leg or arm."

In regard to the ideal recall of movements of the missing member, Weir-Mitchell says:—"We find that in a very small number there is no consciousness of power to stir any part of the absent members by force of will. All others are able to will a movement and apparently to themselves to execute it more or less effectively, although in most of the amputated such phantom motions are confined to the fingers or toes, which rarely seem to possess the normal range either of flexion or extension. Yet the certainty with which these patients describe the limitations of motion, and their confidence as to the place assumed by the parts moved, are truly remarkable; while these restricted movements are pretty surely painful, and the effort is apt to excite twitching in the stump. . . . A small number have entire and painless freedom of motion as regards all parts of the hand. 'My hand is now open, or, it is shut,' they say. 'I touch the thumb with the little finger.' 'The hand is now in the writing position,' etc. Between these cases and such as are conscious of an immobile member, every grade of difference as to motion is to be found."

Another very remarkable fact of some importance is that, according to Weir-Mitchell, such sensations may be roused at will and even revived after they have been in abeyance for years, by stimulation of afferent nerves. He says:—"If we faradise the track of the nerve in or above the stump, we may cause the lost fingers and thumb to seem to be flexed and extended, and, what is most remarkable, parts of which the

man is conscious, but which he has not tried to stir for years, may thus be made to appear to move, to his utter amazement. In one case I thus acted on the nerves, so as to cause a thumb which for years was constantly and violently bent in on the palm to straighten out completely. On breaking the circuit without warning, the patient exclaimed that his thumb was cutting the palm again, and the same result was obtained by shifting the conductors so as to put the nerves out of the circuit. . . . In a case of amputation at the shoulder-joint, in which all consciousness of the limb had long since vanished, I suddenly faradised the brachial plexus, when the patient said at once, 'My hand is there again. It is bent all up and hurts me.' These impressions are correctly referred by the patient, so that faradisation of the musculo-spiral, or the ulnar, gives sensations of movement in the related parts."

These effects of faradisation of the nerve trunks are very interesting, and are entirely in accordance with the statements previously quoted to the effect, that the more healthy the stump the less perfect, after a time, becomes the sense of the existence of the limb that has been removed. If a mere blow upon the stump tends to recall such a sensation, how much more should a definite stimulus applied to the very nerves themselves. These facts simply mean, that the kinæsthetic centres in relation with a lost limb are apt to drop into a condition of functional inertia, but that they are easily roused from this state by the advent of afferent stimuli, and that they are all the more powerfully roused when the stimulus applied is in itself powerful. In these statements, however, we are a little taking for granted the correctness of a view as to the nature of these impressions which has not yet been formally considered (see p. 42), although some of the strongest evidence in its favour has already been adduced in preceding pages.

In regard to the varying power of recall of kinæsthetic impressions exhibited by different persons whose limbs have been amputated, this may be due, in part, to varying conditions of the stump as above indicated; and perhaps more especially to varying states of the nerve ends, and the extent to which they are invaded by processes of sclerosis. Beyond all such causes of difference, however, we may expect that

there would be naturally in different individuals, great variations in the power of recalling kinæsthetic impressions, just as an extreme variability is met with in different persons in regard to their power of recalling visual or auditory impressions, as F. Galton has so fully shown in the case of the former. A few instances of such variations will well illustrate this part of our subject.

Galton made a series of careful enquiries to ascertain to what extent different persons are endowed with the power of recalling, or seeing with the mind's eye, distinct images of objects in their natural grouping and colouring, taking as a trial-subject the person's own breakfast table as he or she sat down to it in the morning. The one hundred answers which he received disclosed an extraordinary range of variation in this respect among different individuals, as may be seen by the perusal of his interesting essay on 'Mental Imagery.'[1] One of those who had the highest power of recall answers:—"Thinking of the breakfast table this morning, all the objects in my mental picture are as bright as the actual scene." One of the worst answers:—"No individual objects, only a general idea of a very uncertain kind." While the person who possessed the lowest power of all in this direction, answered:—"My powers are zero. To my consciousness there is almost no association of memory with objective visual impressions. I recollect the breakfast table but do not see it." Other very interesting examples of variation in this power of recalling visual images are cited. Thus Mr. Galton says:—"One statesman has assured me that a certain hesitation in utterance which he has at times, is due to his being plagued by the image of his manuscript speech, with its original erasures and corrections. He cannot lay the ghost, and he puzzles in trying to decipher it. . . . A distinguished writer on metaphysical topics assures me that he is exceptionally quick at recognizing a face that he has seen before, but he cannot call up a mental image of any face with clearness. . . . There are a few persons in whom the visualising faculty is so low that they can mentally see neither numerals nor anything else; and again there are a few in whom it is so high as to give rise to hallucinations."

[1] 'Inquiries into Human Faculty,' 1883, p. 83.

With variations such as these to be met with in regard to the impressions of a vivid sense like that of sight, it is much more easy to explain, by the principle of individual variation, some of the remarkable differences in the power of recalling kinæsthetic impressions met with among the persons examined by Weir-Mitchell.

The cases to which we have just been referring are instances in which kinæsthetic impressions as a whole are revived. There is, however, good evidence to show that "muscular sense" impressions in particular are also capable of being revived in idea. It has been already shown that these are the impressions which almost, if not quite alone, are concerned with the estimation of weight or resistance. If such impressions, however, had not been capable of being revived in idea, we should have no power of discriminating differences in weights when they are lifted in succession, and still less should we have that power which we possess of lifting certain bodies and saying at once what is their approximate weight.

In the former case we have mentally to compare our present feelings of resistance with our memory of similar impressions experienced a few moments before; whereas, the fact of our possessing such powers as are referred to in the second case, shows, that we are capable of acquiring ingrained "standards of weight" by the constant exercise of this faculty, dependent of course upon our power of ideally recalling such impressions, so that at last we are enabled to judge intuitively as to the approximation of bodies to certain ideal standards of weight, just as we are enabled to judge of the approximation of certain presently perceived colours or sounds to certain "standard" visual or auditory impressions. In regard to this point Professor Bain says:[1]—"Absolute weight implies a permanent standard, and a permanent impression of that standard. When I lift a weight, and pronounce it to be seven pounds, I make a comparison between the present feeling and the impression acquired by handling the standard weight of seven pounds or things equivalent thereto. This absolute comparison, therefore, implies the enduring and recoverable sensibility to impressions of resistance, which is also a fact of the human

[1] 'The Senses and the Intellect,' 3rd ed., p. 93.

constitution. . . . A receiver of posted letters contracts an ingrained sensibility to half an ounce, and can say, of any letter put into his hand, whether it produces a sensibility equal to or under the standard."

But it is one of the fundamental positions of physiological psychology which Professor Bain has strongly enforced, that a renewed feeling or idea must depend upon the action of precisely the same parts as the original feeling. On this head he says[1]:—"What is the manner of occupation of the brain with a resuscitated feeling of resistance, a smell, or a sound? There is only one answer that seems admissible. The renewed feeling occupies the very same parts, and in the same manner as the original feeling, and in no other parts, nor in any other assignable manner. . . . For where should a past feeling be re-embodied if not in the same organs as the feeling when present? It is only in this way that its identity can be preserved; a feeling differently embodied would be a different feeling."

The Nature and Origin of "Muscular Sense" Impressions.

Almost all that has been written by others in regard to what I have in this paper spoken of as "kinæsthetic impressions," has been brought forward in relation with the several writers, views concerning the existence and nature of that power in us whereby we are enabled to judge of "resistance" or "weight." This power has been discussed by them under such different names as these:—"muscular sense" (Sir Charles Bell); a "sense of force" (E. H. Weber); a "sens de l'activité musculaire" (Landry); a "conscience musculaire" (Duchenne); a "locomotive faculty" (Sir William Hamilton); the "feelings of movement" (Bain); and the "sense of innervation" (Wundt).

Some of these writers, such as Sir William Hamilton[2] and Wundt,[3] have carefully discriminated the different components of the group of impressions accompanying and resulting from

[1] 'The Senses and the Intellect,' 3rd ed., p. 338.

[2] 'Notes and Dissertations on Reid.'

[3] 'Éléments de psychol. physiol.,' 2nd ed. (trad. franç.), 1886, t. i., pp. 421–426.

movements; others, like Landry[1] and Bain[2] have not always been careful to make such discrimination, but have spoken rather as though all the impressions derived from movement might be included under that endowment which others speak of as the "muscular sense"—a laxity which is all the less likely to recur if the term kinæsthetic should be adopted as a general designation for the whole mixed group of impressions which accompany movement.[3]

From a psychological point of view the highest interest has attached to these discussions, seeing that the sensations of "resistance" which we derive through the exercise of our muscles have been supposed to give us a knowledge altogether apart from that derivable through so-called "passive sensations"—an immediate knowledge, in fact, of an external world or "non-ego."

Those who consider that through the performance of movements we acquire a kind of knowledge which is altogether peculiar, are not precisely agreed among themselves as to the exact mode in which they so acquire it. These, however, are altogether minor differences, and as a class the holders of such views must be separated from another group of psychologists and physiologists who maintain that there is nothing intrinsically peculiar in the sensations and impressions constituting that combination of feelings which comes to us as a consequence of movement.

Omitting minor detail, therefore, we have two points of view in reference to the nature and origin of the impressions whence we derive our knowledge of "resistance" and of

[1] 'Mém. sur la paral. du sentim. d. activ. musc.' 1855, p. 262.

[2] 'The Senses and the Intellect,' 3rd ed., p. 82.

[3] This looseness of phraseology is, in fact, still very common, as may be judged from the following quotation from Ferrier, where he is speaking of the views of Hitzig and Nothnagel. He says (*loc. cit.*, p. 379): "The term muscular sense, as ordinarily understood, and as employed by these authors, is applied to the assemblage of centripetal impressions generated by the act of muscular contraction in the muscles themselves, as well as in the skin, fasciæ, ligaments, and joints." Historical references bearing upon the subject of the "muscular sense" generally, are mainly to be found in the following three places, but especially in the last of them:—Hamilton's 'Notes and Dissert. on Reid'; Bastian's 'Brain as an Organ of Mind,' *Appendix*; and Sternberg, in Pflüger's 'Archiv f. Physiol.' Bd. xxxvii.

"weight," whereby we acquire our knowledge of the different degrees of "consistence" of bodies, and (through consciousness of the nature and extent of our movements) whereby we learn the differences in "form" which bodies present.

These conflicting views must now be briefly but definitely stated, in order that it may be made perfectly clear which of them is in accordance, and which at variance, with facts that are now well established:—

(1.) According to the first view our notions of "resistance" are not derived from sensations at all, but are due to a central psychic process (to which Wundt has applied the term "sense of innervation"). In reference to such a view Ludwig says:—"It is conceivable and not unlikely, that all knowledge and discrimination arrived at through the exertion of the voluntary muscles are attained directly through the act of voluntary excitation; so that the effort of the will is at once proceeded on as a means of judgment." On this subject also, J. Müller said:—"It is not certain that the idea of the force employed in muscular contraction depends solely upon a sensation. We have a very exact notion of the quantity of nerve-force starting from the brain which is necessary to produce a certain movement. . . . It would be very possible that the appreciation of the weight and pressure in cases where we raise or resist, should be in part at least not a sensation in the muscle, but a notion of the quantity of nerve-force which the brain is excited to call into action." According to Wundt, "the strength of the sensation is dependent only on the strength of the motive influence passing outwards from the centre, which sets on the innervation of the motor-nerves." Bain, again, holds "that the sensibility accompanying muscular movement coincides with the outgoing stream of motor-energy, and does not, as in the case of pure sensation, result from an influence passing inwards, by ingoing or sensory nerves," or, as he elsewhere says, "we are bound to presume that this is the concomitant of the outgoing current by which the muscles are stimulated to act." Hughlings-Jackson, amongst ourselves, has always been a strong upholder of these views.

(2.) The opposite view is not so old. The earliest statement that I have seen to the effect that the sensations accompanying

or resulting from movement are afferent and not efferent feelings, is that of Lotze, who says:[1]—"We must affirm universally that in the muscular feeling we are not sensible of the *force* on its way to produce an effect, but only of the *sufferance* already produced in our moveable organs, the muscles, after the force has, in a manner unobservable by us, exerted upon them its causality." This view was, however, very shortly after, much more clearly expressed by Landry[2] in these terms:—"The *ego* has a direct consciousness of the phenomena of volition; it knows immediately that there has been a voluntary stimulus, and to what part of the body it has been directed; as to the effects produced, it is only mediately informed of these, and can disregard them. . . . The nervous action which incites the movement can only, therefore, furnish to consciousness an idea of the volition, and not that of its execution; besides, if the sensorium has a knowledge of the excitation of the excito-motor faculties, it ignores the quantity of the nerve action exerted. It is necessary that the effect of the central incitation (the contraction) should be produced in order that the brain may perceive, and then it perceives at the same time both the seat and the degree of contraction. The movement is, therefore, the source whence we derive notions of this kind."

In 1869 I expressed the view,[3] that "*all* the information which Prof. Bain considers to have been revealed to us by the activity of our locomotive organs through an 'active' mode of sensibility, has been derived from modes of passive sensibility (occasioned, it is true, by this activity), and from inferences which have gradually been built up upon these . . . The feeling of 'expended energy,' therefore, by which we obtain our ideas of resistance and of an external world, is not contained, as we think, in the volitional act itself, but is derived from impressions emanating from the moving organs themselves during the actual accomplishment of movements." And after referring to the clinical evidence that was available

[1] 'Medicinische Psychologie,' 1852, p. 293.

[2] 'Traité des paralysies,' Paris, 1859. Similar views were indeed expressed in his memoir in the 'Gaz. des Hôpitaux,' 1855.

[3] "On the 'Muscular Sense,' and on the Physiology of Thinking." 'British Medical Journal,' April, 1869.

at that time, concerning cases in which the so-called "muscular sense" was supposed to be more or less defective, I arrived at the following conclusion :—"There is clearly a loss of something in these cases, of a something which serves as a guide in the execution of voluntary movements, but whose absence can be compensated by the supervision of the visual sense; and this is, in great part, the function which some physiologists attach to the 'muscular sense' . . . my position is that these impressions of the muscular sense, whose existence we are thus obliged to postulate, are *unconscious*[1] impressions, and that the conscious impressions that have usually been stated to fall within its province are really derivable through modes of ordinary cutaneous and deep sensibility."

In 1880, whilst adhering to almost precisely the same views, I maintained[2] that the time had come when it would be advantageous to speak, in lieu of a "muscular sense," of a "Sense of Movement, as a separate endowment, of a complex kind, whereby we are made acquainted with the position and movements of our limbs, whereby we judge of 'weight' and 'resistance,' and by means of which the brain also derives much unconscious guidance in the performance of movements generally, but especially in those of the automatic type. . . . There are included under it, as its several components, cutaneous impressions, impressions from muscles and other deep textures of the limbs (such as fasciæ, tendons, and articular surfaces), all of which yield conscious impressions of various degrees of definiteness; and in addition there seems to be a highly important set of 'unfelt' impressions, which guide the motor activity of the brain by automatically bringing it into relation with the different degrees of contraction of muscles that may be in a state of action."

Meanwhile Ferrier had also expressed a strong opinion adverse to the views of Wundt and Bain.[3] He said :—"But, whether we make the movements in reality, or revive them in idea, the consciousness of the extent and energy of the move-

[1] In regard to this point, see what I have said on p. 5 (Note [3]) of this paper.
[2] 'The Brain as an Organ of Mind,' pp. 540–544.
[3] 'Functions of the Brain,' 1st ed., 1876, p. 226.

ment is, in my opinion, in all cases dependent on in-going or centripetal impressions."

A few weeks after the publication of my work 'The Brain as an Organ of Mind,' there appeared a remarkable paper[1] by Dr. William James, of Harvard University, on 'The Feeling of Effort,' in which will be found many views strikingly similar to those that I have expressed. In reply to the question as to the nature of our sensible perceptions of movement he says:— "I unhesitatingly answer: an aggregate of afferent feelings, coming primarily from the contraction of muscles, the stretching of tendons, ligaments and skin, and the rubbing and pressing of joints; and secondarily, from the eye, the ear, the skin, nose or palate, any or all of which may be indirectly affected by the movement as it takes place in another part of the body. The only idea of a movement which we *can* possess is composed of images of these its afferent effects." Again he says:— "The *degree of strength* of our muscular contractions is completely revealed to us by afferent feelings coming from the muscles themselves and their insertions, from the vicinity of the joints, and from the general fixation of the larynx, chest, face and body, in the phenomena of effort, objectively considered. When a certain degree of energy of contraction rather than another is thought of by us, this complex aggregate of afferent feelings, forming the material of our thought, renders absolutely precise and distinctive our mental image of the exact strength of movement to be made, and the exact amount of resistance to be overcome."

Such language may seem very positive, but in my opinion also this second view as to the afferent nature of all the impressions by which we obtain a knowledge of our movements and by which we judge of weight and resistance is one that may now be considered to be incontestably established. Sachs[2] has shown the existence of sensory nerves in muscles, and such clinical evidence as is afforded by the cases of Demeaux, Landry, Spaeth, Duchenne, Briquet, and Bazire, show conclusively what opinion we ought to adopt in regard to this much disputed point. We have to do in these patients with

[1] 'Anniversary Memoirs of the Boston Society of Natural History,' 1880.

Reichert's u. Du Bois-Reymond's 'Archiv,' 1874.

a total disappearance, in the parts affected, of all that kind of knowledge which has, by one or other, been ascribed to, or supposed to be derived from, the "muscular sense." They were ignorant of the position of their limbs, unconscious of any movements that they might execute, and of the degree of resistance which might be opposed to the contraction of these muscles. The volitional centres, the spinal motor centres, the motor nerves and the muscles were capable of being called into activity—yet all the information usually supposed to be derived through the "muscular sense" had vanished. No clearer evidence could be forthcoming to show that the knowledge of the position of our limbs, of their movements, and of the state and degree of contraction of our muscles generally, does not depend as Wundt, Bain, and others assume, upon impressions that are "concomitants of," or that coincide with, "the outgoing stream of nervous energy."

I go even further, and maintain that the processes taking place in motor centres and in motor nerves are purely physiological processes, wholly devoid of any conscious accompaniment—that is to say, from the time the motor incitation leaves the kinæsthetic centres, up to the time when muscular contractions occur, we have only to do with a series of physical processes occurring in different motor centres and in motor nerves.

Writing on this subject in 1880 I said[1]:—"The kinæsthetic centre is indeed one of great importance. Its impressions enter inextricably into a large number of our mental processes—as widely and inextricably, in fact, as the assumed muscular consciousness of Bain is supposed by him and others to be intertwined with what they would distinguish as passive sensibilities. But it can only produce an extreme amount of confusion if the activity of this sensory centre is attributed to and confounded with that of motor centres, the processes of which seem to lie even more truly outside the sphere of mind than the molecular processes comprised in the actual contraction of a muscle: these latter processes are, at least, immediately followed by 'ingoing' impressions, whilst so far as we know—that is, so far as any evidence exists—the former are

[1] 'The Brain as an Organ of Mind,' p. 600.

not." After so much as we are conscious of in the mere volition itself, "we have to do with molecular currents passing, it may be, through several sets of fibres and cells, but having no conscious side whatever."[1] Again, "Motor centres, wherever they may be situated, are parts whose activity appears to be wholly free from subjective concomitants. No ideal reproductions appear ever to take place in such centres; they are roused into activity by outgoing currents, and so far as we have any evidence, the induction in them of molecular currents which immediately afterwards issue through cranial and spinal motor nerves to muscles are simply physical phenomena."[2]

Views almost exactly similar to these were published only a few weeks later by Wm. James, in his essay on 'The Feeling of Effort.' Thus, he says (p. 20):—"In a word, volition is a psychic or moral fact pure and simple, and is absolutely completed when the *intention* or *consent* is there. The supervention of motion upon its completion is a supernumerary phenomenon belonging to the department of physiology exclusively, and depending on the organic structure and condition of executive ganglia, whose functioning is quite unconscious." Or again (p. 4):—"But apart from *à priori* postulates, and however strange to logic it may appear, it is a fact, that the motor apparatus is absolutely insentient in an afferent direction, although we know that the fibres of the anterior root will propagate a disturbance in that direction as well as in the other. Why may not this result from a true insentiency in the motor cell? an insentiency which would accompany all action there, and characterize its normal discharges as well as the unnatural irritations made by the knife of the surgeon or the electrodes of the physiologist upon the motor nerves." Again (p. 7)—"One immediate conclusion follows: namely, that there are no such things as afferent feelings, or feelings of innervation. These are wholly mythological entities. Whoever says that in raising his arm he is ignorant of how many muscles he contracts, in what order of sequence and in what degrees of intensity, expressly avows a colossal amount of unconsciousness of the processes of motor discharge."

[1] *Loc. cit.* p. 495. [2] *Loc. cit.* p. 593, see also p. 149.

I am happy to be able to cite another distinguished convert to these views. If any one will compare what was said by Prof. Ferrier in the first edition of his 'Functions of the Brain'[1] with what he says in the following passages taken from his recently-issued second edition (p. 436) it will be seen that he has made a most important renunciation, and now holds doctrines entirely in accordance with those above expressed. The views he now holds are these:—"The activity of the motor centres has no subjective side apart from the functioning of the sensory centres with which they are associated . . . whereas we have sensory ideation in and by itself, we have no ideas of movement apart from the sensory centres through which alone the activity of the motor centres is revealed in consciousness."

Many important issues in regard to cerebral localisation, as well as concerning psychological doctrine, turn upon the correct interpretation of the questions which we have just been considering.

In regard to the former, with which we are here chiefly concerned, it may be well to point out in the first place, that the interpretation of aphasia, aphemia, and all the varied amnesic states must differ widely from one another according to the different views which are taken as to the nature and seat of so-called "motor-ideas." The very notion of such a thing as a "motor-idea" could only arise in the mind of a man who believed we had feelings which could be felt, and

[1] At p. 266, he said:—"In the same manner as the sensory centres form the organic basis of the memory of sensory impressions, and the seat of their representation or revival in idea, so the motor centres of the hemispheres, besides being the centres of differentiated movements are also the organic basis of the memory of the corresponding movements, and the seat of their re-execution or ideal reproduction. We have thus a sensory memory and a motor memory, sensory ideas and motor ideas; sensory ideas being revived sensations, motor ideas being revived or ideal movements." Again at p. 271, he said:—"If the motor centres of the cerebral hemispheres are not merely the centres of impulse of volitional movements, but also the centres of registration and revivability of the same, it must follow that destruction of the cortical motor centres will cause not only objective motor paralysis, but subjective motor paralysis, or, in other words, paralysis of motor ideation." These views which Ferrier has, I am glad to say, renounced are thoroughly in accordance with doctrines still held by Hughlings-Jackson, and Stricker.

consequently ideas that could be revived, in motor centres. It so happens also that those who hold this doctrine have always been inclined to believe (though it does not of necessity follow), that such revived "motor ideas" constitute the material of our recollection when words recur to the mind in silent thought; this, for instance, has been the doctrine of Bain, Wundt, Hughlings-Jackson, and more recently of Stricker. Those, however, who repudiate the notion of the existence of any such things as "motor ideas," and who believe that all our ideas of movement are revived in sensory centres, have also for the most part supposed, that words are primarily revived in silent thought in the auditory centres, and that the kinæsthetic ideas of words are awakened in immediate succession thereto.

These divergent views lead to widely different interpretations of the various species of speech defects, and such interpretations entail quite different views as to the localisation of the lesions by which they are severally caused. This any one may see who will take the trouble to compare Stricker's account of such defects, as embodied in his work[1] 'Du langage et de la musique,' with that which I have advanced in a recently published work.[2] The different interpretations we have offered of so-called "word-blindness" and "word-deafness" might be looked to as an example of what I mean. The whole question is, however, far too large a one to be entered upon here.

Again, if our sensations of movement were "concomitants of the outgoing current," and if we really had "motor ideas" as well as sensory ideas, it would be perfectly natural and logical to look for motor centres in the cerebral cortex; but if these views are erroneous, and if all our knowledge of movements comes to us through sensory centres, then there is no longer the least ground for postulating the existence of motor centres in the cortex; all that would be needed there would be the existence of sensory registers of the impressions occasioned by movement. In other words, kinæsthetic centres clearly must exist in the cerebral cortex, to be as it were on the same platform with the registers for other sensory impressions with

[1] Paris (Biblioth. de philosoph. contemp.), 1885.

[2] 'Paralyses: Cerebral, Bulbar, and Spinal,' 1886, pp. 99–130.

which they are in such intimate functional relationship; whilst the actual need for the existence of motor centres in this situation has not yet been shown by any one.

The Part taken by "Muscular Sense" Impressions in the Execution of Voluntary and Automatic Movements.

(*a*) *Voluntary Movements.*

In voluntary acts we have not merely to account for the production of a movement, but of a movement of a certain kind, in which the action of each set of muscles brought into play is duly regulated so as to lead to the exact result desired. And, seeing that the volition or desire to bring about such and such movements originates in the cerebral cortex, so we are bound to admit, that the purely volitional qualities of the movement must also depend upon cerebral influence. Hence the strength, the continuance, the rapidity, and the direction of movements are variable according to the precise nature of the cerebral incitation or volition. As Jaccoud says, "The strength of the movement is regulated by the strength of the initial motor impulse; the extent depends in reality upon the same influence; the rapidity results from the more or less rapid succession of voluntary impulses; and the direction is determined by the voluntary localisation of the incitation upon certain groups of muscles." It is also important to recollect that, in the execution of a complex movement, any alteration that we may desire to bring about in respect of any one of these volitional qualities, is, by the mere change in the volition itself, immediately effected with reference to the movement as a complex whole. Jaccoud illustrates this as follows[1]:—"I am walking, and then I wish to walk more quickly. Hardly have I conceived the desire before the mode of walking is changed; it has become in short, more rapid. This intervention of my will manifests itself, therefore, by a change in the movement of locomotion as a whole. This change is the final result of a series of partial modifications which have been brought about in the original movement; this is incontestable. But I have

[1] 'Les paraplégies et l'ataxie du mouvement,' 1864, p. 594.

not needed a parallel series of volitional acts; a single volition has sufficed. I have willed to walk more quickly, and I have then walked more quickly, without knowing anything, without even requiring to know anything, of these intermediate modifications." In the same way, alterations in any one of the other volitional qualities of a complex movement are found to have direct bearings upon the movement as a whole. These seem to be the principal facts that should be mentioned with regard to the mere volitional act; and I am quite disposed to agree with Landry when he says, in opposition to the views of several physiologists previously mentioned, that the volition itself includes only an incitation to a specific kind of movement, and that we must execute this movement in order to become acquainted (inferentially) with the quantity of nervous action brought into play.

But besides these *qualities* of the movement which are determined by the volition, we have to consider the execution of the movement itself as a compound of several simple movements, brought about by the simultaneous and successive contractions of different muscles, which are perfectly harmonious and constant in their mode of action. This machine-like precision of action—the result of what is usually known by the name of *co-ordination*—is now admitted by most physiologists to depend upon certain pre-established, though gradually acquired, nerve-connections between the different elements of the spinal cord and medulla oblongata. The movements are machine-like, inasmuch as they depend upon certain organic combinations in the spinal cord and medulla, and after these connections have once been fully formed the will has little more to do with them; the movements ordinarily take place in a definite manner, the will qualifying their mode of execution only. It requires a special exercise of mental power in order to execute certain complex movements in a way different from that which has become habitual. But in these cases, as well as when we are learning to execute a new complex movement, we give no attention whatever to the muscles by which the movement is effected—the states of these individually do not fall within the scope of our consciousness. Were it otherwise, the number and situations of the several muscles would reveal

themselves to all, during the performance of different movements. In our tentative efforts we think only of the movements themselves—how to combine the more simple, so as to produce the more complex.

The kind of movement produced is, therefore, evidently dependent upon the *distribution*, in the medulla and spinal cord, of the volitional impulse; and, in a complicated motor act, its incidence upon particular groups of cells, which are the organic representatives of certain potentialities for simple movements, gives rise to the production of the complex movement. The more frequently such a complex movement has been executed, the more completely may we suppose these various groups of cells become bound together into one system by connecting fibres, and the more possible is it for the movement which they represent to occur in a thoroughly automatic manner, and often without much need for cerebral intervention. The mechanism of co-ordination is, therefore, purely spinal. This fact was fully recognised by Volkmann more than forty years ago, as appears from the following quotation:[1] "The physiological accomplishment of movements has nothing to do either with consciousness or with unconsciousness, for the mind has not the least notion of the details of this operation; and even in voluntary movements it knows nothing concerning the nerves or the muscles by the intervention of which the process is accomplished. In fact, the mind does nothing in this case but give the incitation; and if this incitation has for an effect any co-ordinated movement, it is altogether simply because the organ which receives the incitation is arranged in such a fashion that it necessarily produces a co-ordinated movement." Precisely the same views as these were also expressed, at about the same time, by Arnold and Müller.

Thus, the work of co-ordination tends to become entirely spinal and organic; the movement itself depends upon the spinal cord, though its particular *qualities* of force, rapidity, etc., are dependent upon the cerebral or volitional influence. The cerebrum may, therefore, also be said to exercise a kind of co-ordination—it co-ordinates or adapts the movements which are

[1] Art. 'Nervenphysiologie,' in Wagner's 'Handwörterbuch,' 1844.

organically represented in the spinal cord, so as to make them accord qualitatively with the aim conceived. But, in order that the cerebrum may exercise this power, it seems perfectly obvious that it should be instructed from moment to moment as to the exact nature of the movement actually produced, so that it may know whether to continue in its present mode of action, or whether to vary the quality of the volition, in order better to attain the desired end. Now, according to Jaccoud, whose treatment of this subject is most excellent, the cerebrum obtains these necessary guiding impressions from different sources. He says: "These indispensable notions the brain obtains directly through the sense of sight; or, instead it deduces them indirectly from the instructions which reach it as to the situation of the parts which move and the condition of the contractile organs which move them; these instructions are furnished by impressions through the muscular sense, and through the sense of touch." The sense of sight is what we chiefly rely upon in early years, and in acquiring new movements generally. We all know how long it is before a person learning to play upon a musical instrument can do without the aid of this guiding sense. At last, however, his tactile sense, and also his "muscular sense," has become so educated that he is able to do without guidance from the sense of sight; and, as a rule, he does without this primary aid as soon as the execution of the movement has become perfectly easy. Jaccoud calls the second mode of appreciation indirect, because it does not, as in the case of the sense of sight, depend upon a simple perception of transmitted sensations, but upon an interpretation of sensations. As he says[1]—"The sensorium requires, as a preliminary, to have learned the relations which unite the various conditions of the muscles, or of the tactile organs to the different sensations perceived; it is only at the termination of this apprenticeship that it can conclude from the sensation perceived as to the statical or dynamical conditions of the parts whence the sensation springs. This education proceeds correctly, by means of the direct appreciation through the sense of sight, thanks to which the individual can compare at each instant the movement effected with the sensation perceived."

[1] *Loc. cit.* p. 601.

After this preliminary education has been finished, the knowledge so acquired, though inferential, becomes as available and as efficacious as that which is more directly derived through the sense of sight. It can be brought into action also with just as much rapidity and exactness, so that the sense of sight is no longer needed to inform us as to the position of our limbs, and as to the nature and degree of their movements.

Jaccoud, therefore, thinks that the power which healthy persons enjoy of performing, with facility, movements that are at the same time habitual and complex depends, in the first place, upon the integrity of their "muscular sense," and in the second, upon that of their sense of touch. Impressions are derived through both these channels, whereby the brain becomes informed as to the amount and kind of movement which it has called into action; and so it learns whether, to obtain the end in view, it should continue with the same kind of volition, or whether it should be qualified in any way.

When we commence a movement, we initiate it with certain predetermined qualities of force and extent; and this, of course, is simply a result of our past experience and education. I know that certain objects have hitherto given me certain impressions of weight when I have previously handled them, and therefore my previous education now enables me, when I see such an object again and desire to handle it, to give the volitional act its necessary qualifications. This power has been termed "*l'instinct locomoteur*," and "*conscience musculaire*." It has been made the subject of much mystification, though it is a simple result of our capability of recalling in idea the kinæsthetic impressions which have previously been associated with given sight impressions; that is, the sight impressions immediately recall their related kinæsthetic impressions. When I see a simple bundle of wool on a table, as a result of previous experience I can at once nearly accurately determine what ought to be the quality of the volition necessary to enable me to raise it. Thus I am enabled to initiate such a movement as I deem appropriate. But supposing, in the case just cited, that the supposed simple bundle of wool was not a simple bundle, that it contained a heavy leaden weight in its centre, then my initial volition would have been inadequate, I should have

been deceived, and the kinæsthetic impressions that I received would have instructed me that a stronger volitional effort was necessary. It is always in this way that kinæsthetic impressions are supposed to intervene.

By a supposed "locomotive instinct" (or, in other words, ideal recall of kinæsthetic impressions), we know, as Jaccoud puts it, *what force we ought to employ*, whilst by the actual occurrence of kinæsthetic impressions we are taught *what force we have employed.*

I have now for some time advocated such views as these, and have maintained that the immediate execution of voluntary acts, in the case of the majority of limb-movements, is dependent upon the guidance of co-active visual and kinæsthetic centres; just as in the case of the complex movements concerned in articulate speech, the immediate execution of such movements is dependent upon the regulative activity of combined auditory and kinæsthetic centres.[1] The latter movements seem, in short, to be related to the auditory centres, in just the same sort of way that movements of the limbs are related to the visual centres.

Where the movements which it is desired to execute are complex and difficult and we have to learn them by imitation of the movements of other persons, the sense of sight is doubly brought into action. It is necessary at the commencement and during the continuance of our efforts to copy such movements, to look alternately at our model and at our own moving members. A long time and much practice is, in fact, required before a person who is learning to play upon some musical instrument, is able to execute the necessary actions without the aid, from moment to moment, of guiding visual impressions. During the process of learning, therefore, the visual centre evidently exercises a dominating influence.

In time, however, the impressions pertaining to the "sense of movement" (which are, of course, always associated with those of sight) become, by way of newly organized channels, sufficiently well associated with the newly organizing motor mechanisms, to permit the new movement whenever it has been initiated to be continued under the immediate guidance

[1] 'Brit. Med. Journal,' April, 1869, and 'The Brain as an Organ of Mind,' p. 555, and chap. xxix.

of kinæsthetic impressions only—that is, without further necessity for a conjoint direction through the sense of sight.

The same different tracts of the brain that are called into simultaneous or immediately successive activity for the initiation of any set of voluntary movements, would probably remain in activity during the continuance of such movement, though not exactly in the same relative proportions. Thus, if we suppose the centres specially called into activity, as guiding centres, to be the visual and kinæsthetic, it may well be that the former has a dominating influence in the production of the initial conception of the movement about to be executed. And yet the distinctness of this idea or conception of the movement (partly visual and partly kinæsthetic in its origin, as we have said) will be found to vary with the degree of familiarity, and consequently with the ease of execution, of the movement. In the case of the simplest voluntary movements, or those that have been often repeated, an idea or conception of the movement needed scarcely obtrudes itself at all as a conscious element of the volition. This is a part of the process which has here become more or less latent, and which in ideo-motor and sensori-motor actions has become wholly latent; though it probably still remains, even in these latter, as a necessary link in the chain of causation. On the other hand, during the continuance of voluntary as well as of almost all varieties of automatic movements, it seems clear that the kinæsthetic centres exercise the supreme guiding influence. Its impressions alone—even when they very imperfectly, or not at all, rouse our consciousness as to their existence—suffice to inform us (that is, suffice to excite their proper cerebral "centres" in ways definitely related to different positions and muscular tensions) as to the exact relations of our limbs, and as to the nature and degree of their movements.

The mode of acquisition above indicated, seems well to accord with our other interests and with the daily necessities of our lives. The sense of sight greatly facilitates the process of learning, and its vivid impressions speedily enable the brain to appreciate aright the more vague and occult impressions coming to it simultaneously through the kinæsthetic centres. Soon, however, the visual sense, which we need for so many

other important purposes, no longer requires to be concentrated wholly on the performance of movements. Later still, our attention or consciousness becomes further freed from disturbing details connected with movements. The possibly conscious impressions pertaining to the "sense of movement" at last habitually pass unheeded, and then we come to be able to perform multitudes of daily actions under the guidance of mere "unconscious" kinæsthetic impressions.

Thus the working of the motor side of our complex nervous mechanism, even when it is concerned in executing the behests of will, proceeds so smoothly, and is practically so much unheeded, as to leave us free to follow up the threads of our conscious life unhindered by the multitudinous details pertaining to the varying states of innumerable muscles acting in ever changing combinations.

Thus from what has been said it seems clear that the performance of a voluntary act is always preceded by an idea or conception of the movement we desire to execute; and that this idea or conception is, for ordinary movements, compounded of two kinds of past impressions, namely, those of the visual sense and those of the kinæsthetic sense. Again, it must be remembered that the kinæsthetic sense includes two different sets of impressions; the one set (*a*) being conscious impressions derived from our moving members (proceeding from skin, muscles, tendons, and joints); the other being (*b*) either unfelt, or almost completely unfelt, impressions emanating from the muscles, in relation with their varying states of contraction, and therefore affording information to the brain of the most important kind. The evidence demonstrating the existence of this last set of comparatively unfelt impressions emanating from muscles, and pointing to their importance for the guidance of movements, is mainly derived from cases of diseases such as have been already cited (pp. 10–20).

Now, in the chapter on "The Will," in his "Analysis of the Human Mind," published more than fifty years ago, it appears that James Mill clearly appreciated the fact, that the "idea of the action" to be performed is two-fold. He says, "There are two ideas very different from one another, to both

of which we give the name 'idea of the action.'" Of these, he adds, "one is the outward appearance of the action, and is always a very obvious idea." The other is a copy of certain internal sensations, which a few pages before he had spoken of generally as sensations accompanying the movement, and which he also more specifically defined (*loc. cit.* p. 275) when speaking of the terminal events of a movement as "the contraction of the muscles, with the various sensations which the action upon those organs, and the action excited in them, imply." Of these internal sensations he says, "from the habit of not attending to them, we have lost the power of attending." And then he adds, "This last (namely the revival of such internal sensations) is by no means an obvious idea. And the mind passes from it so quickly, intent upon the action which is its result, that it is almost always swallowed up in the mass of association. It constitutes, in fact, one of the most remarkable instances of that class of links in a chain, which, how important soever to the existence of the chain, are passed over so rapidly, that the existence of them is hardly ever recognized. . . . *This last idea alone is that upon which the contraction is consequent.*"

This view is then essentially that which I myself adopt, viz., that kinæsthetic impressions, and especially those of which we are least conscious, are the last to be revived in the cerebral cortex, anterior to, and as actual last links in the chain of cerebral processes which determine, the excitation of the motor centres themselves.

I find that since the first publication of these views, very similar notions as to the importance of revived sensations of movement for the execution of voluntary acts have been expressed by two other writers.

The views of one of these, namely Dr. E. Fournié, were published three years afterwards;[1] and though we do not at all agree in our general interpretation of perceptive processes, nor as to the regions of the brain concerned therewith, we are thoroughly in accord as to the importance of these sensations of movement in conjunction with other sensory impressions as guides for movements generally. Thus, he fully

[1] 'Physiolog. du syst. nerveux,' 1872, pp. 291, 360, 362-5, 447

recognized the important part played by revived sensations of muscular contraction in voluntary as well as in instinctive movements; also the difficulty of reviving such impressions alone, when they have not been linked with visual or auditory impressions. It is true, he does not seem clear as to the mode in which such revived sensations of movement come into play, though he firmly believes that conjoined sensory impressions are the true cerebral co-ordinators of movement, and that by the aid of these alone we are enabled to learn new movements. He holds that sensations telling of the state of contraction of the muscles are "indispensable conditions in order that each movement may be directed in a suitable fashion," and says that, but for our power of recalling in idea or reviving such impressions, new movements could never be perfectly learned, so that the "execution of movements would be an eternal apprenticeship."

But much more important and more generally in accordance with my own views are the statements made by Dr. W. James. He says[1]:—"The essentials of a voluntary movement are—(1) a preliminary idea of the end we wish to obtain; (2) a '*fiat*'; (3) an appropriate muscular contraction; (4) the end felt as actually accomplished. In man at any rate it is admitted that the idea of the end and the muscular contraction were originally coupled by empirical association; that is to say, the child with his end in view, made random movements till he accidentally found one to fit. This movement awakened its own characteristic feeling which thenceforward remained with him as the idea of the movement appropriate to that particular end.[2] If the man should acquire a million distinct ends, he must acquire a million such motor ideas and a million connections between them and the ends. But one such connection, subserved by an exclusive nerve tract used for no other purpose, will be enough for each end. The end conceived, will, when these associations are formed, always

[1] *Loc. cit.*, 1880, p. 5.

[2] In a note further on, Dr. James calls attention to an important point. He writes (*loc. cit.*, p. 9):—"I may add that in teaching a new and unnatural movement, the starting-point is to awaken by its passive production a distinct sense of what the movement, if effected, would feel like. This defines the direction of the exertion the pupil is to make."

awaken its own proper motor idea. As for the manner in which this idea awakens its own proper movement—the one which will convert it from an idea into an actual sensation—the simplest possible arrangement would be to let it serve directly (through its peculiar neural process) as a stimulus to the special motor centre, the ultimate sensible effect of whose discharge it prefigures and represents." Again, he says:—"If I will to write 'Peter' rather than Paul, it is the thought of certain digital sensations, of certain alphabetic sounds, of certain appearances on the paper, and of no others, which immediately precedes the motion of my pen. If I will to utter the word 'Paul' rather than Peter, it is the thought of my voice falling on my ear, and of certain muscular feelings in tongue, lips, and larynx, which guide the utterance. All these feelings are afferent, and between the thought of them, by which the act is mentally specified with all possible completeness, and the act itself, there is no room for any third order of mental phenomena. Except indeed what I have called the fiat, the element of consent, or resolve that the act shall ensue. This, doubtless, to the reader's mind, as to my own, constitutes the essence of the voluntariness of the act."[1]

But the so-called "fiat" has no particular mystery about it. It is the mere result of the fact, that one out of the two or more motives which weigh themselves against one another, is, after little or much deliberation, recognised to be stronger than the others; the result being that the molecular movements with which it is associated are permitted to flow over into motor channels so as to evoke the fitting muscular actions. As James says:—"In our bed we think of the cold, and we feel the warmth and lie still, but we all the time feel that we can

[1] In regard to the nature of this "fiat" we may say, with James Mill, that it is simply equivalent to a *desire* sufficiently strong to be immediately operative (see 'The Brain as an Organ of Mind,' p. 550). Hartley said also, "The Will is, therefore, that desire or aversion which is strongest for the present time." Which mental mood is to prevail is sometimes immediately settled, and at other times only after a process of Deliberation. Concerning this process Hobbes said:—"The whole sum of desires, aversions, hopes and fears, continued till the thing be either done or thought impossible, is what we call *Deliberation*. . . . Appetite, therefore, and aversion are simply so called as long as they follow not deliberation. But if deliberation have gone before, then the last act of it, if it be appetite, is called *will*; if aversion, *unwillingness*."

get up *if we will*. The difficulty is to will." In the case of emotional actions, the mere presence of the exciting idea is much more apt immediately to excite the corresponding movement, so that "the discharge of idea into movement is much more readily inhibited by other casually present ideas in the case of voluntary action, and less so in the case of emotions; though here too inhibition takes place on a large scale." As a result of these considerations he lays down the following important conclusion,[1] "*that every representation of a motion awakens the actual motion which is its object, unless inhibited by some antagonistic representation simultaneously present to the mind.*"

W. James very neatly summarizes his own as well as my views on this part of the question, when he says:—"The ordinary 'voluntary' act results in this way: First, some movement produces a feeling in a reflex, or as we say, accidental way. The movement excites a sensorial tract, causing a feeling which, whenever the sensorial tract functions again, revives as an idea. Now the sensorial and motor tracts, thus associated in their actions, remain associated for ever afterwards; and as the motor originally aroused the sensory, so the sensory may now arouse the motor (provided no outlying ideational tracts in connection with it prevent it from so doing). Voluntary acts are, in fact, nothing but acts whose motor centres are so constituted that they can be roused by these sensorial centres, whose excitement was originally their effect."

Finally, it would seem clearly to follow, from the views above set forth, that in all essential respects the cerebral mechanisms for the actual production of voluntary, of ideo-motor, and of emotional movements are identical—that is to say, that the "way out" from the sensory centres in which the idea of the movement to be effected is revived, and the efferent tracts thence onwards towards the motor centres by which the movement is to be actually evoked are, when the movement itself is similar, identically the same in each of these cases.

The sensory components of the idea of the action about to be performed are, moreover, always twofold. They either belong to the visual and the kinæsthetic orders, or else they are auditory and kinæsthetic, as in the case where speech movements are to be evoked. The cases are rare in which, with ordinary seeing

[1] *Loc. cit.* p. 17.

individuals, we have the combination in idea of special tactile with kinæsthetic impressions. In the blind, however, this particular combination for the ideal recall of movements becomes very common.

In each case the "conception of the movement" about to be executed, being a means rather than an end, does not after a time attract our attention. If this is so even with the initial guides of movement (that is, the revived visual and auditory impressions), how much more may we expect it to be the case with the revived kinæsthetic impressions, which are always less vivid. And how little even the initial guides of movement, when revived in idea, at all clearly reveal themselves to consciousness in many persons is evidenced by the fact, that it is denied by Prof. Stricker and some other persons that words are primarily revived, anterior to articulation, in the auditory centres.

It is, however, neither customary nor needful either for thought or for action, that the several links in the chain of association by which these processes are effected should reveal themselves in consciousness. Many of the links of our thought and of our sensorial action are represented by sub-conscious nerve processes, and this is clearly the case with regard to those linked revivals of sensory activity which immediately precede and guide muscular movements. Transitions from conscious to unconscious nerve actions are habitually taking place during the education of the individual and the development of the nervous system in each one of us; and yet the essential nerve actions either still go on in the latter case, and none the less surely exercise their influence, in evoking other nerve actions which are associated with conscious states, or in leading to the production of this or that form of movement.

(*b*) *Automatic Movements.*

As we have seen, between voluntary and ideo-motor or emotional acts no really intrinsic differences exist, from the point of view of the nerve centres concerned with their production.[1]

[1] In the first edition of his 'Functions of the Brain,' Dr. Ferrier distinctly argued as though the nerve mechanisms needful for the production of voluntary

Are we to hold, however, with Ferrier and others, that there are special motor centres for the performance of voluntary actions altogether apart from those by the aid of which automatic actions are performed? This is a view to which I cannot subscribe. An automatic movement is simply what was once a voluntary movement in the race from which the individual has sprung. The motor mechanisms that are now immediately concerned with its execution are precisely those which were originally linked together or "organized" when such a movement first began to be effected by voluntary efforts.

The difference here, as I contend, is not with the motor centres; it lies rather with the nervous processes which precede the action of the motor centres. Thus, when eating, one swallows food by a series of automatic actions, without taking heed of the process; but I can at any time, by directing my attention thereto, and with the presence of saliva as an additional stimulus, voluntarily perform these same processes. In the one case the movements are evoked as mere reflex actions in which the medulla is the centre concerned; in the other they are evoked under the influence of volitional stimuli whose birth and starting-point is to be found in the cerebral cortex.

It is, therefore, in my opinion, a fundamental error to look for special motor centres for the production of voluntary movements of any kind—either in the cortex or elsewhere.

The cerebral hemispheres are needed for the learning of new movements which in succeeding generations of animals come to be performed in a purely automatic manner; or, we ourselves may learn movements which ultimately by dint of time and long practice may become so facile that they take their place in the category of "secondary-automatic" actions. These facts seem to me to show conclusively that, in the learning of any new movement, two processes of education (entailing the establishment of structural connections, or the laying down of new nerve routes) must take place concurrently.

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movements were different from those by which ideo-motor and emotional movements are evoked. In the recently issued second edition he does not so explicitly state this view (*loc. cit.*, p. 371), nor does he clearly retract it.

One must be carried on in the cerebral cortex in the way already described; whilst another must be effected by developmental processes taking place in lower centres, of the action of which we are altogether unconscious—processes which, with lapse of time and successive generations, ultimately permit such movements to occur in a purely automatic fashion, and, it may be, without any concurrent stimulus either reaching or rousing the activity of the cerebral cortex.

It has now been perfectly well established, by experiments on lower animals, that even such complex muscular actions as are required for station and locomotion may be performed in pigeons and in rabbits after the cerebral hemispheres have been removed. Here, therefore, it is clear we have only to do with a series of very complex reflex actions, carried on by means of the mesencephalic and cerebellar centres. It is true that afferent impressions of various kinds are needed for the effective performance of the very complex muscular actions required for the maintenance of equilibrium and for locomotion—there must, in fact, be the advent of proper visual impressions, of labyrinthine impressions, of tactile impressions, and above all of "muscular sense" impressions. With the sole presence of these, however, the necessary movements for locomotion may be evoked, even though in its natural condition the animal is also in the habit of constantly inciting these same complex movements by volitional stimuli. The motor centres in action remain the same, they are merely called into play in a different manner; and in each case the actual movement, whether reflex or volitional, is evoked by stimuli starting from sensory centres.

For the execution of many other automatic movements the kinæsthetic impressions which impinge upon lower centres are of less importance than tactile impressions; these latter, in many cases, are the special stimuli which evoke the acts, whilst kinæsthetic impressions here also serve as absolutely unconscious guides—especially where the act is one which involves a succession of muscular movements. It must be upon the basis of information coming to the spinal cord itself in the form of kinæsthetic impressions, that, even in animals in which the cerebral hemispheres have been removed, as Goltz has shown,

we may at times observe new adaptive movements called forth of a distinctly purposive type, that is, with a view to remove some unusual stimulus. This is an adaptive power which, as Dr. Ferrier admits,[1] the evidence compels us to concede to the spinal cord.

This kind of use to which kinæsthetic impressions are put in the case of automatic movements, holds good for movements of different degrees of complexity, and equally so whether they are evoked under the guidance of mesencephalic, of medullary, or of spinal sensory nuclei.

But by the time a new movement has been thoroughly learned by any particular race of animals, that is, after its constant repetition by successive generations of such animals, these tactile and kinæsthetic impressions to lower centres (whether they be spinal, medullary or mesencephalic) having been habitually impinging upon such centres (as well as making their way to the cerebral cortex) must have opened up direct channels of communication between such lower sensory centres and the related motor mechanisms, so that the corresponding movements are then capable of being evoked in a purely reflex fashion, after the manner of automatic movements generally—that is, on the mere occurrence of the suitable stimulus, and without necessity for the intervention of the cerebral hemispheres. It is true that, in the majority of cases, the cortical connections with such motor centres still exist, so that the same movement may, whenever the desire is felt, continue to be evoked by a voluntary stimulus. This, as we have seen, is the condition of things with regard to the movements of deglutition already referred to, as well as for station and locomotion in many of the lower animals.

Experiments upon lower animals seem, indeed, to teach us that if we take different creatures such as frogs, rabbits, dogs,

[1] 'Functions of the Brain,' 2nd ed., p. 118, where he says:—"By a series of ingeniously contrived experiments Goltz has shown that, even when the limbs of a frog were so fixed or placed in positions which could not have occurred in its past experience, the animal, without its hemispheres, retained the power of adapting its movements in accordance with these unusual and abnormal conditions. This would indicate that, if these centres are merely centres of reflex action, the reaction is that of a machine possessing in some way the power of self-adjustment."

matic movements, and that these imaginary "motor centres for voluntary movements" are situated in the cerebral cortex.

The Cortical Localisation of "Muscular Sense" Impressions.

We have seen that there are practically three groups of cases in which loss or defect of "muscular sense" impressions is apt to be met with (p. 21):—(*a*) cases where there is disease in the posterior columns or posterior cornua of the spinal cord; (*b*) cases of cerebral hemianæsthesia, in which there is functional or structural disease in the region of the posterior extremity of the internal capsule; and (*c*) cases which I previously spoke of as a more ill-defined group, but which I will now venture to say are cases of disease in the so-called motor regions of the cerebral cortex.

In regard to the precise path through the spinal cord taken by muscular sense impressions, nothing definite can as yet be said, beyond what has been above indicated. It is principally with some forms of locomotor ataxy, among spinal diseases, that defects of "muscular sense" are met with, therefore we suppose these impressions must pass upwards either through some part of the posterior column or else through the posterior cornua of the cord.

We are also unable definitely to trace the path pursued by "muscular sense" impressions through the medulla, pons, and cerebral peduncle. The fact, however, that in a very large proportion of cases of cerebral hemianæsthesia no loss of the muscular sense exists while, in other rare cases, these special impressions as well as any others that may be derived from muscles are entirely absent, tends to make it probable that in or near the posterior part of the internal capsule the channels for these impressions are gathered together in a more or less distinct fasciculus, before they diverge on their way to the cerebral cortex.

The final question that now presents itself for consideration, however, is:—What are the parts of the cortex to which these "muscular sense" impressions proceed? Can this be said to be known, or not?

or monkeys, we find in the first mentioned a comparatively small number of movements that are not purely automatic, and consequently a very small number of these which are permanently interfered with by a removal of their cerebral hemispheres. In rabbits and dogs, however, we find movements more and more interfered with after removal of the cerebral hemispheres, showing, according to my interpretation of the facts, that a larger proportion of the movements of these animals are as yet unemancipated from the necessity of cerebral control and guidance in the manner I have indicated through sensory centres. In monkeys, again, we meet with a large increase in the proportion of movements which are still so comparatively new (for the race), as to be only capable of being evoked by stimuli emanating from the cerebral cortex. While in man, the knowledge we have acquired from the study of disease, as well as our knowledge of the infinitely diversified uses to which the human hand may minister, suffices to convince us that the influence of the cerebral cortex in the production of movements even very greatly surpasses that which obtains with the monkey.

Thus, we find in man the kinæsthetic centres, as well as the visual, auditory, and tactile centres, alike obtaining their maximum development, because of the extremely diversified uses and the complexity of the processes to which they are called upon to minister. The very fact of the greater development of all these sensory centres in man, and of the enormous number of the movements which are suggested and guided by his intelligence, probably tends to maintain movements generally in him more, and for a longer time, under the influence and guidance of the cerebral cortex, than similar movements would be in lower animals. Hence the larger amount of motor defect produced in man by certain cortical lesions, simply because these lesions destroy those sensory centres in the cortex from which the volitional stimuli or controlling incitations immediately issue.

This is a kind of explanation which seems to me much more harmonious with all the facts than that advanced by Dr. Ferrier and others, which supposes that there are motor centres for voluntary movements altogether distinct from those for auto-

medulla, and cord, receive their ultimate guiding stimuli), the following result ought to ensue :—

	Classes of Movement.	Results.
Destruction of "Muscular Sense" Centres in One Hemisphere.	Movements of Class I. . .	All of them paralysed.
	Movements of Class II. . .	These movements not paralysed.

With practice under such conditions, and after some time, it is probable that some of the movements of Class I. which were nearest akin to those of Class II. would become capable of re-execution. They would pass indeed from the one class into the other—so that some amount of recovery from paralysis might be brought about in this way.

Now let us, in the same way, consider the effect of *stimulation* either of all or of some of the "muscular sense" centres in one hemisphere, and ascertain the effects that should be induced in regard to the same two classes of movement :—

	Classes of Movement.	Results.
Stimulation of "Muscular Sense" Centres in One Hemisphere.	Movements of Class I. . .	Contractions or spasms affecting the muscles by which these movements are performed.
	Movements of Class II. . .	Do. do.

Evidence obtainable from observations on Man.

From destruction or stimulation of the "muscular sense" centres in man, we may look for just the same kind of effects, in reference to the two classes of movement, as we have above said might be expected to occur in the lower animals. There should be a difference in degree only, dependent upon the fact of the far larger proportion of movements which, in man, belong to Class I., as compared with Class II. As a matter of fact (thanks in great part to the experimental investigations of Dr. Ferrier), we are now all familiar with the paralyses that result from destructive lesions in the Rolandic area of the cortex, and with the various forms of "Jacksonian Epilepsy" resulting from irritative disease in these same regions.

Other defects, however, may be looked for in the case of man, over and above those of which we can obtain distinct evidence from animals, as a result of destruction of his

In seeking to obtain an answer to this question, we ought to look for certain evidence that may be obtainable from experiments upon lower animals, and we should supplement this information by other evidence of a clinico-pathological nature derived from the effects of disease of the cerebral cortex in man, or from the effects resulting from removal of portions of the cortex on account of disease in this situation.

Evidence obtainable from lower animals.

What information may we fairly seek to obtain on this subject from experimentation upon the lower animals, and especially upon monkeys? From what has been said as to the functions of the "muscular sense" centres in the cortex in reference to movements generally, it seems obvious that they must be divided into two classes, from the point of view of the degree of their relationship to the activity of the cerebral cortex. Thus we have :—

Class I.—Voluntary actions in the largest sense of that term, among which we would include all those movements of varying degrees of facility (that is ranging from new difficult movements, to those of ideo-motor type) which cannot be executed apart from cortical instigation and guidance, proceeding from the opposite cerebral hemisphere.

Class II.—Actions that are only at times incited voluntarily (that is, by incitations from the cerebral cortex), but which are, in the great majority of cases, evoked by the reflex activity of lower centres as true automatic movements.

It is necessary to bear this distinction in mind because, as will be seen, the results of destruction of the "muscular sense" centres in the cerebral cortex on the one hand, and of their stimulation on the other, should be different for the two classes of movement.

Supposing, for the sake of our argument, we have *destroyed* the whole of the "muscular sense" centres of one hemisphere (the centres through which, in all actions that are performed voluntarily, the motor centres of the opposite side of the pons,

"muscular sense" centres. These are certain defects of sensibility of such a kind that it would be quite hopeless to attempt to obtain any clear and reliable evidence concerning them by experiments upon, and observation of, any of the lower animals.

It is all the more necessary to insist upon this point, because some experimenters have drawn conclusions from their observations as though it were possible to obtain such evidence from the lower animals. But, I would ask, how is it possible for us to form any trustworthy judgment concerning a blindfolded animal's knowledge (1) of the extent of the active or passive movements of one of its limbs, or of the exact position in which it may be left at the termination of these active or passive movements; or how, again, (2) are we to judge whether the animal's ability to discriminate differences in weight or resistance with one of its limbs has suffered any diminution? These are much the most valuable means of testing the integrity of the "muscular sense," yet no evidence can be obtained in regard to either of them which is of the least value, except from an animal that has the power of speaking and telling us what his impressions are under the various trial circumstances. The only other kind of test (of less value than the two above-named, because it leads to more equivocal results) is (3) the testing of the animal's ability to perform certain definite movements with one of its limbs when its eyes are closed. How difficult it must be to obtain any reliable information upon such a subject by observations upon a blindfolded animal can be easily imagined, if we look only to the limited range of movements of a suitable kind which the animal could be induced to perform under such conditions. This, however, is not the whole extent of the difficulty that we should have to face in such an investigation; there is the further complication, that the animal's limb which it would be most important to test in regard to the integrity of its "muscular sense" impressions, in the particular experiments with which we are concerned, would be partially paralysed, and thus all attempts to test the integrity of its muscular sense impressions by this third and only means which is available in animals would be rendered still more fallacious and untrustworthy.

I am strongly of opinion, therefore, that all the statements which have been made in reference to the integrity or otherwise of special "muscular sense" impressions, in any of the lower animals that have been operated upon, are altogether valueless,[1] and that we must look only to observations upon man to throw any true light upon this side of the question.

The additional defects which ought to be met with in man, however, as a result of disease of the "muscular sense" centres are of this kind. Destructive disease, or removal by the surgeon, of parts of the "muscular sense" centres should lead, in proportion to the completeness of the destruction of the parts, to a more or less marked loss of that kind of knowledge which comes from the "muscular sense" in connection with the parts of the limb that are paralysed, or the whole limb or limbs, as the case may be. In a patient in whom ordinary tactile sensibility is not much affected, if we find, after various random passive movements with eyes closed, that he cannot tell correctly the position in which his limb or fingers may be left, the evidence becomes all the stronger that there must be a notable defect in the "muscular sense," because, as may be supposed, in such a case the patient may derive some guidance towards such knowledge through skin impressions. In these cases, therefore, the loss of the "muscular sense" would *always seem to be less than it really is*, and unless great care be taken it would be easy for a superficial observer to come to the conclusion, that it was not impaired at all. One precaution, for instance, should always be taken. That is, the observer, after passively moving the patient's limb or fingers, must be careful not to let his own fingers remain in contact with the patient at the end of these passive movements, when he asks as to the position in which the limb or fingers may have been left. If, however, we should have our suspicions that part of a patient's ability to tell the nature of passive movements made, or of the position in which his limb is left, comes from the guidance that he derives through the tactile sense, we must resort to other methods before we arrive at a positive opinion as to whether the "muscular sense" is defective or not.

[1] See note to p. 9.

In doubtful cases, therefore, perhaps the most trustworthy means of arriving at a positive conclusion as to the integrity or otherwise of this sense are these three (neither of which would be applicable, be it observed, in the case of one of the lower animals):—(*a*) Ascertaining whether the patient has or has not a complete knowledge of the position of the limb or limbs in question on first awaking from sleep, and before they are moved in any way; (*b*) Testing his ability to appreciate differences in weight with the part affected[1]; and finally (*c*) we may resort to a method adopted by Westphal and also by Horsley (a method which is particularly useful when the part to be tested is much paralysed), that is, ascertaining when the patient's eyes are closed with what readiness, or the reverse, he may be able to bring the forefinger of the sound hand at once upon any particular part that may be indicated to him (though not touched) of the affected limb in a state of rest. With a defective knowledge of the presence and position of a limb, the power of at once finding a particular part indicated would probably be distinctly impaired.

In brief, then, it comes to this, that if we have regard to the functions of the "muscular sense" department of the kinæsthetic centres (of which they form the nucleus or most essential part), we should expect that their stimulation would lead to spasms and convulsive movements of the parts whose motions are in relation therewith; that destruction of such centres would, on the other hand, lead to a paralysis of those movements which have hitherto only been evoked by the action of these centres; and, further, that in this latter case, there would also be a loss or marked diminution of "muscular sense" in the parts principally affected.

It will at once be seen that these are precisely the effects which have now been ascertained to follow upon the stimulation and destruction of the so-called "motor centres" in the cerebral cortex, the position of which, thanks to the successive labours of Fritsch and Hitzig, of Ferrier, of Carville and Duret, of Exner, of Horsley and Schäfer, and of Beevor and Horsley, have now been very thoroughly ascertained. The

[1] With specially charged gun cartridge cases after Galton's method, or some other equally delicate means. (See 'Inquiries into Human Faculty,' 1883, p. 370.)

position of the centres in connection with different movements of the limbs and trunk have now, in short, been definitely localised by these various workers as well as by clinical observers, in different parts of the Rolandic area and of the marginal convolution.

My opinion, therefore, is that the evidence in our possession points very strongly to the conclusion, that Ferrier's so-called "motor centres" are in reality kinæsthetic centres in which "muscular sense" impressions in particular have been registered. The following reasons lead me to this conclusion:—

1.—All the effects resulting from the stimulation or destruction of these centres are, as I have already shown, in accordance with this view.

2.—This being so, the view that "motor centres" exist in the cerebral cortex cannot be correct, unless it can be shown that there is in the cortex of each hemisphere another totally distinct set of centres, the stimulation of which evokes definite movements, and the destruction of which involves an inability to execute the same movements. But, both experimental physiology and clinical medicine speak strongly to the fact, that there is but one set of areas (Rolandic and marginal) in which irritation or destruction leads to any such results. I claim, therefore, that these areas must be in great part (whatever other functions they may discharge) devoted to the registration of kinæsthetic impressions of the "muscular sense" order, which, as I have endeavoured to show, are so all-important for the production of voluntary movements.

3.—No valid reasons have ever been brought forward against this view. To this it may be replied that Dr. Ferrier has carefully examined such doctrines, in the last edition of his 'Functions of the Brain' (pp. 379–381), and that he has there shown how much such views are at variance with existing knowledge.

The necessities of the case compel me, therefore, to be frank concerning Dr. Ferrier's position in regard to, and his treatment of, this question, to which I have given considerable attention.

According to Hitzig[1] the so-called motor centres are the

[1] Reichert's u. Dubois-Reymond's 'Archiv,' 1870 and 1874; also 'Untersuch. in das Gehirn,' p. 59, 1872.

centres for the "muscular sense" or "muscle-consciousness," while according to Nothnagel,[1] though not the centres for, these regions are traversed by, impressions of this order. Both, however, adopt that lax view of the "muscular sense" which would make it include impressions from joints, skin, etc., or, in other words, all the impressions resulting from movement, which I have grouped together as kinæsthetic impressions. Thus their views, although nominally the same as mine, are really different, seeing that I am content to subscribe to the views of Ferrier as well as of Horsley and Schäfer, to the effect that the falciform lobe (hippocampus, gyrus hippocampus, and gyrus fornicatus) is the main seat, at all events, in which tactile impressions and those of common sensibility are registered. The convolutions in the excitable Rolandic area together with the marginal convolution, are, however, according to my view, the seats in which "muscular sense" impressions proper (the all-important constituents of kinæsthetic impressions) are registered, and from which volitional stimuli immediately issue. Arguments, therefore, which may be valid against the views of Nothnagel and Hitzig, in regard to the non-impairment of common sensibility in cases of destructive disease of the parts in question, may be of no avail against my doctrine.

As, however, we have much in common, it may be well to scrutinise Dr. Ferrier's attitude with regard to these views as a whole, especially as he does not endeavour to make any nice or essential distinctions between them.

Thus, he does not attempt to distinguish between the effects that might be expected to follow from loss of the "muscular sense" centres, and those which would result merely from the cutting off of the proper impressions from such centres, they themselves being left intact. Any one who has read the present paper will realize that from my point of view this is a most important distinction. In his first edition Ferrier pointed out that Nothnagel regards the cortical centres in question as "in some manner directly connected with the paths" of muscular sense impressions, while Hitzig regards them as the cortical termini for these impressions. In his second edition, he does not think

[1] Virchow's 'Archiv,' Bd. lvii. 1873.

it worth while to state any such distinction; he assimilates Nothnagel's doctrine to that of Hitzig, and, strange to say, assumes that the effect of the destruction of such centres would be ataxy rather than paralysis. He even repeats this as against my doctrine, saying, "It might explain ataxy, but not paralysis." This, as well as other remarks which he makes, convinces me that Dr. Ferrier has given no adequate consideration to the subject. Let me say definitely that, in accordance with my views, paralysis should result from the destruction of these centres; and that inaccurate and more or less disordered movements when the eyes are closed (ataxy) should be the effect of the cutting across of the paths of muscular sense impressions whether near the hinder part of the internal capsule or in the spinal cord.[1]

Ferrier, treating the question from the point of view of the mere cutting off of muscular sense impressions (which is in fact Nothnagel's doctrine, though not Hitzig's which he is ostensibly criticising), supposes, therefore, that the resulting defects would be of an ataxic order more especially, and then goes on to say that this is, at all events, not the nature of the defect met with in the monkey or in man. Then comes the following very positive statement:—"It is also certain—and a subject of daily clinical demonstration—that in paralysis from cortical disease the patient though unable to move his arm voluntarily, is perfectly aware of every movement passively communicated to it, and can state with exactitude whether his arm is flexed or extended, whether his fist is closed or open, and whether his finger is being flexed or extended gently or with force. His muscular sense, as well as every other form of common sensibility, is absolutely unimpaired." How very far this is from being an accurate statement, however, any one may judge

[1] In relation with this statement, it may be well here to recall the fact, that if a low condition of functional activity in the "muscular sense" centres should co-exist with a disease which cuts off its proper impressions from such centres, there may, in addition, be inability to perform the simplest movements when the eyes are closed (pp. 16–20 and 36); while, in other cases, with a still lower state of functional activity of the "muscular sense" centres (with or without co-existing hemianæsthesia), we should have to do with paralysis of a *hysterical type*, in which voluntary movements cannot be performed either with eyes closed or with eyes open (p. 36).

who will refer to the cases which I have previously cited (pp. 24–32), viz. the case of P. Zenner, presumably one of cortical injury, as well as the two cases recorded by V. Horsley, in which he had excised portions of the cortex. Afterwards, in his attempts to dispose of the doctrines of Hitzig and Nothnagel, Dr. Ferrier makes a series of statements which, to say the least, are, as I have attempted to show in previous parts of this paper, very insufficiently founded upon fact and actually misleading in their nature.[1]

In short, it seems to me perfectly obvious that Dr. Ferrier has never thoroughly thought out this portion of his subject, and that his expressed opinions have been, and still are, most inconsistent. He began, in the first edition of his justly celebrated work, by assuming that motor centres were seats in which the ideal recall of movements occurred[2]—though at the same time, and in flagrant opposition therewith, he strongly contended that all sensations resulting from movement reach the brain through afferent channels.[3] In the second edition of his work he has corrected this discrepancy. He now admits that the ideal recall of movements can only occur in sensory centres—though he appears to do all he can to explain away the importance of these kinæsthetic centres. He shows no gleam of recognition of their importance for the execution of voluntary movements.[4] He, indeed, expressly repudiates their influence in this direction (and the opinions of those who support such a notion) principally by means of two assertions which are almost wholly at variance with clinical teaching. Thus, he makes the extraordinary statement, that loss of tactile sensibility carries with it loss of the muscular sense;[5] and, further, that loss of the muscular sense never occurs alone, or, as he implies, otherwise than in direct proportion to loss of tactile sensibility.[6]

Dr. Ferrier's statements concerning the order of events in voluntary acts are, moreover, extremely inexplicit; his assumption that the mechanisms for such acts are organised in

[1] I mean the statements which I have quoted in Notes to pp. 15 and 23.
[2] See (*loc. cit.*) the first paragraphs of § 92, § 96, and § 98.
[3] *Loc. cit.* pp. 218–228.
[4] *Loc. cit.*, 2nd ed., pp. 432–438.
[5] See note on p. 22 of this paper.
[6] See note on p. 23 of this paper.

special cortical centres, rests on just as unsatisfactory a basis as does his notion, that all kinæsthetic impressions are mixed inextricably with tactile impressions in one and the same cortical terminus.[1] As this, in fact, is a rival hypothesis, I ought to quote it in Dr. Ferrier's own words, more especially as it is all, so far as I have been able to find, that he says throughout his work and on his own account concerning the cortical localisation of "muscular sense" impressions. After remarking that he considers it established "beyond all doubt that the falciform lobe is the centre of common and tactile sensibility," he adds[2]:—"In the same regions are also the centres of cutaneo-mucous and so-called muscular sensibility. . . . All the facts receive the most satisfactory explanation, if we regard the falciform lobe as a whole, and in each and every part the centre of tactile sensation for the whole of the opposite side of the body; though probably the various motor centres are each anatomically related by associating fibres with corresponding regions of the falciform lobe. This association would form the basis of a musculo-sensory localisation."

In the few remarks which Dr. Ferrier has made in regard to my views in particular,[3] he principally confines himself to a statement of the difficulties which he experienced in comprehending their precise meaning. These difficulties I trust he will now no longer experience; at least I have striven to explain my meaning as clearly as possible. Several of his statements concerning these views are not altogether correct, but one which he makes is extremely misleading, in more ways than one, and calls for some notice from me. He leads his readers to believe, that my views should not find favour because they are based upon an old and erroneous notion, that "the cortical centres act only through the corpora striata." How far this is from being a correct representation of my position the reader may judge for himself, seeing that this is the first occasion on which any mention of these basal ganglia has been made in this paper. My views are, in fact, wholly independent

[1] This latter conclusion is based upon deductions made from his observations upon animals. See note on p. 9 of this paper.

[2] *Loc. cit.*, 2nd ed., p. 344.

[3] *Loc. cit.*, p. 381.

of any fixed notions concerning the functions of the corpora striata, as to which so much doubt has arisen of late years.[1]

Facts which favour or oppose the Notion, that the Centres in the Rolandic and Marginal Areas are real Motor Centres.

Having adduced the reasons which induce me to believe, that the so-called "motor centres" of the cortex are in reality the cortical termini of muscular sense impressions, and having discussed, and I hope shown, the futility of some of the objections which have been made to this view, I will now briefly glance (*a*) at the facts upon which reliance is placed by others in support of the notion, that these centres are really motor, and then turn (*b*) to an enumeration of the strong reasons which, in my opinion, can be brought forward against any such hypothesis.

(*a*) The first facts to be considered are the results of stimulation or destruction of the regions in question.

In regard to these it will be well, first of all, to quote what Prof. Ferrier says. He writes as follows:[2]—"As regards the physiological significance of these regions, we have seen that we cannot conclude, from the mere occurrence of movement on the electrical stimulation, that the regions are truly motor; for the stimulation of a sensory centre may give rise to reflex or associated movements. . . . Whether the centres now under consideration are directly motor, or only give rise to movements in a similar reflex, or indirect

[1] This criticism comes, however, all the more strangely from Dr. Ferrier, seeing that in my work 'The Brain as an Organ of Mind,' 1880, I certainly could not have said anything more definite about the functions of the corpora striata than he said in the first edition of his 'Functions of the Brain,' 1876, where I find the following statements:—"The cortical motor centres which necessarily act downwards through the corpus striatum" (p. 210); "The corpus striatum is the centre in which movements primarily dependent on volition proper tend to become organised" (p. 214); "In these cases, and in the dog deprived of its cortical centres, the path from impression to action is not, as in the ordinary course of volition, through the cortical motor centres to the corpus striatum, and thence downwards," etc. (p. 215). These were the statements concerning the corpus striatum which were backed by Dr. Ferrier's authority till about two months ago, when the second edition of his work appeared.

[2] *Loc. cit.*, 2nd ed., p. 317.

manner when stimulated, is a question which has been answered differently by different physiologists. The definite purposive character clearly perceivable in most of the movements, however, their correspondence with the ordinary volitional activities and individual peculiarities of the animals, and above all their uniformity and predicableness, harmonize best with the hypothesis, that they are the signs of the artificial excitation of the functional activity of centres immediately concerned in effecting volitional movements, *and as such truly motor.* If these centres are part of the mechanism of volitional movements, then paralysis of voluntary motion, *and of motion only,* ought to result from their destruction, and any apparent exception must be capable of satisfactory explanation in accordance with this view, if it is the correct one." This is in my opinion a thoroughly just statement in all respects, except in regard to the two passages which I have caused to be printed in italics.

To the first passage so printed I altogether demur, as being a complete *non sequitur.* The movements excited might also be expected to have all the characters described by Dr. Ferrier, if they were sensory centres "immediately concerned in effecting volitional movements," viz., those in which "muscular sense" impressions are registered. It is, in fact, a rule which obtains throughout the nervous system, that all motor centres whatsoever are always stimulated into activity through incitations coming from sensory centres or nuclei, consequently the electrical stimulation of such centres or groups of cells, or of the fibres issuing from them, should always evoke just such movements as the excitation of these centres or cells is accustomed to produce. Similarly, destruction of such centres or nuclei should render impossible all such movements as had previously been evoked by their agency as necessary factors.

Secondly, if "these centres are part of the mechanism of volitional movements," and if they subserve such functions as I have imagined, the second passage which has been underlined would also be altogether wrong; their destruction, in that case, would involve not only paralysis of voluntary motion but also loss of the so-called "muscular sense" in the related parts.

So far, then, there is nothing which may not be explained by my hypothesis with just as much ease as by the more fashionable notion. We come, however, now to another set of facts, which are pointed to triumphantly by Ferrier and his adherents, as a crowning proof of the truth of their notions, viz. to the fact, that the study of descending degenerations in the pyramidal tract conclusively shows, that efferent fibres pass continuously from the centres in the Rolandic and marginal areas downwards to motor centres in the medulla and throughout the whole length of the spinal cord.

To this I reply, that the direction of secondary degenerations affords good evidence as to the efferent or afferent functions of the nerve fibres which are so affected, and that our present knowledge shows conclusively that the fibres in the pyramidal tract are efferent fibres—a notion which I hold equally with Dr. Ferrier. This, however, does not at all touch the question, whether the ganglion cells which exercise a trophic influence upon such efferent fibres form constituent parts of sensory or of motor centres, which is the real question in dispute. If this trophic influence is a mere collateral incident of the functional activity of the cell, as is now generally supposed,[1] then it must always be exercised in the direction taken by currents starting from the cell, so that the fact, that secondary degenerations in the pyramidal tract pursue a descending course, merely tells us that we have to do with efferent nerves, and absolutely nothing as to whether the trophic cells from which they issue belong to sensory or to motor centres.

The direction of the degeneration says nothing, therefore, which is more in favour of Ferrier's than of my hypothesis.

Again, the fact that these pyramidal fibres pass directly downwards from the centres in question in the cerebral cortex to groups of motor cells situated in the pons, the medulla, and throughout the spinal cord, is a point which tells no more in favour of the one hypothesis than of the other.

Thus, it is an essential part of my general view (which I stated so long ago as 1869) to suppose, that the elementary motor mechanisms which are called into play in voluntary

[1] And as Dr. Ferrier himself imagines (*loc. cit.*, 2nd edit., p. 85).

movements are the same, and no other, than those which have to be called into action in corresponding automatic movements (see p. 65). This view has lately been strengthened by some valuable experimental researches of Ferrier and Yeo, concerning which the former says[1]:—"The stimulation of each motor root of the nerve plexuses of the limbs in monkeys, calls forth combined movements involving the co-operation of numerous muscles, widely separated from each other anatomically, but all resulting in actions such as are seen to be constantly associated together in the ordinary modes of activity of the animal." After giving illustrations of this, Ferrier adds:—"These facts render it probable that each segment of the cervical and lumbar enlargement of the spinal cord, whence nerves proceed to the limbs, is a centre of co-ordinated synergic muscular movements, of a character adapted to the habits and requirements of the animal in its ordinary modes of activity." Ferrier's recent work, therefore, gives a positive basis of support to the view which I had previously enunciated, and have lately reiterated,[2] though it seems to me to tell rather against his notion that there are separate motor centres in the cortex, on the ground that the spinal mechanisms merely require excitations from the cerebral cortex in order to evoke purposive movements.

Either view as to the functions of the cortical centres would still, and equally, leave in doubt the question, whether or not other centres in any way co-operated with these and the medullary or spinal centres in the bringing about of new and complex voluntary acts. Of course we know that, where station and locomotion are concerned, the cerebral incitation must be in part devoted to a rousing of activity in the conjoined mesencephalic and cerebellar centres. Again, supposing the thalamus and corpus striatum to constitute a higher couple still for the performance of acts which are not voluntary, as Dr. Ferrier suggests, we may ask what are the efferent fibres through which these centres act upon those below, and we may put the same question in regard to the combined mesencephalic and cerebellar centres. Such questions become all the more

[1] *Loc. cit.*, p. 76.

[2] 'The Brain as an Organ of Mind,' 1880, p. 558.

pertinent if applied to the case of a dog which has had its so-called "motor centres" removed from both hemispheres, and which, if it should live long enough, as in Carville and Duret's experiments, would presumably have secondary degenerations throughout both pyramidal tracks. Yet in such animals, after a time, both station and locomotion become possible.[1] Here, therefore, the incitation of all the motor nuclei needful for the accomplishment of the complex muscular contractions occurring during locomotion must take place through channels other than those pertaining to the degenerated pyramidal tracts. I make these latter remarks merely for the purpose of indicating how much we still have to learn upon this whole set of questions, and not with the view of showing that such difficulties press more heavily upon the one than upon the other of the two hypotheses now being weighed in the balance.

The above-mentioned are the only real grounds that have ever been put forward in support of the notion, that the centres in question in the Rolandic and marginal areas of the cortex are true "motor centres."[2] What has been said above, however, clearly shows that the facts admit of a totally different interpretation. If we put aside for the moment the question, whether the "muscular sense" is or is not impaired or lost in limbs, less or more paralysed from lesions occurring in these portions of the cerebral cortex, all the remaining facts are, to say the least, fully as much in accordance with my view as with that of Dr. Ferrier. If, however, subsequent observations on man should confirm what the observations of Horsley and Zenner render possible or even probable, that loss or impairment of the "muscular sense" will be found to exist in the paralysed parts in cases of cortical disease, then the balance of evidence would be completely turned in favour of my hypothesis, that these Rolandic and marginal centres are the cortical termini for "muscular sense" impressions. This would be crucial evidence which could not be gainsaid even by the

[1] Ferrier, *loc. cit.*, p. 367.

[2] The mere large size and other peculiarities of the nerve cells in these regions of the cortex may be explained by the one as well as by the other hypothesis, if we look to the nature of the stimuli issuing from them, and to the length of the paths which such stimuli would have to traverse.

most sceptical, since as Dr. Ferrier himself puts it, in accordance with his view that the above-mentioned cortical centres are true motor centres, "paralysis of voluntary motion, and *of motion only*, ought to result from their destruction."

(*b*) So far, however, I have only been discussing the value of the evidence which its supporters put forward in favour of their notion, that the cortical centres above indicated are true motor centres; it still remains for me to state, what can and ought to be said against the notion of the existence of any such centres in the cerebral cortex. Whilst, in fact, the additional evidence from observations on man, alluded to above, may be needed to convince the strongest partizans of the "motor" hypothesis as to the truth of my particular view, I trust many persons may be found to agree with me that, even in our present state of knowledge, the following considerations are amply sufficient to show, not only that there is now no independent foundation for the hypothesis that "motor centres for voluntary action" exist in the cerebral cortex, but that such a hypothesis is in reality repugnant alike to physiological and to psychological data.

The theory which first led to the postulation of the existence of such centres by Dr. Hughlings-Jackson has been thoroughly disproved, and nobody admits this more fully than does Dr. Ferrier himself. Hughlings-Jackson has always strongly supported the view of Bain as to our feelings of movement being "concomitants of the outgoing current," and that such feelings are ideally revivable in motor rather than in sensory centres. The logical corollary of these views is, undoubtedly, that motor centres should exist in the cerebral cortex. As we have seen, Dr. Ferrier has, one by one, dropt these fundamental doctrines as erroneous, and yet he still clings to a theory which is the natural associate of such rejected doctrines. His position is a very inconsistent one.

The supposition that motor centres exist in the cortex for the performance of voluntary movements, is, however, thoroughly repugnant in itself to what we may term the physiologico-psychological analysis of the volitional act. This teaches us that sensory centres are the real guides of volitional action; that their activity corresponds with the very essence of volition;

that they, in fact, do just such work as that which those who do not adequately think out the problems involved attribute to cortical motor centres.

Ferrier's view is equally repugnant to the physiology of movements from an evolutional standpoint, which teaches us that the self-same motor mechanisms which are at first called into play volitionally (that is, by incitations from cortical sensory centres), become, with lapse of time in new generations of animals, the motor mechanisms for corresponding automatic acts. Ferrier's view, that the centres in the cortex are motor, expressly says that they are for the accomplishment of volitional acts only; and at the same time he tells us nothing as to how this assumed registration of voluntary movements, in regions altogether distinct from those in which automatic movements are registered, can be reconciled with the fact, that every sort of transition exists between voluntary and automatic movements, and that many of the self-same movements are at times executed volitionally, though at other times they are performed as typical automatic movements. If he does not mean to say that the voluntary acts are registered in the cortical centres as actual motor mechanisms, then I maintain that, in consequence of an imperfect analysis of the phenomena of volition, he is simply attributing to assumed motor centres that guiding action which is really carried on in sensory cortical centres.

Finally, there is only one complete set of excitable areas in the cortex of each hemisphere (through which movements may be evoked in all parts of the body), but if Dr. Ferrier's views were true there ought to be two complete sets of such excitable centres in each hemisphere. Clearly and indubitably the sensory incitations to movement, constituting (as all admit) part of the volitional act, *must pass off from certain cortical areas in a definite and orderly manner in order to excite motor centres, wherever they may be situated.* I assume that they pass off in such fashion from the cortical termini for "muscular sense" impressions. The stimulation of these centres or efferent fibres, which must exist, should, therefore, clearly be capable of evoking purposive volitional movements. All those who, contrary alike to the teachings of psychology and of

physiology, would look for separate volitional motor centres in the cerebral cortex must seek to discover therein another distinct set of excitable areas. Perhaps it might be easier and more conducive to sound doctrine for them to reconsider the whole question?

It may be supposed by some to be, after all, a matter of no importance, whether we call these areas in the Rolandic and marginal regions of the cerebral cortex "motor centres" or not, especially seeing that, as I myself admit, they are the areas whence volitional motor incitations issue, so that their efferent fibres really convey motor incitations.

To this position I altogether demur. It is, in fact, a matter of the highest physiological and psychological importance that the correct view should be recognised and that a nomenclature which was, as I am disposed to think, entirely based upon an incorrect and mistaken view should be altogether discarded. There should be no half measures; the old view is either right or wrong, and if wrong it cannot be right to designate certain sensory areas of the cortex either as "motor" or "psycho-motor" centres, even though they are the cortical termini of ingoing impressions resulting from movements. I would put it therefore in this way:—

1. For the sake of physiological consistency, we should not call a cortical centre for afferent impressions "motor," *any more than we should call the cell nuclei on the sensory side of a spinal reflex arc "motor."* In each case they give birth to fibres which convey motor impulses, and in each case the stimulation of such internuncial fibres would give birth to definite movements.[1]

2. Again, the retention of any nomenclature which implies that the excitable areas in the cerebral cortex are "motor centres," tends to foster false physiological and psychological doctrines such as these:—

[1] As I have elsewhere said ('The Brain as an Organ of Mind,' 1880, p. 585):—"The plan on which nerve centres generally are constructed, of whatsoever grade, makes it essential that the stimulus which awakens the activity of a 'motor' ganglion or centre shall come to it through connecting fibres from a 'sensory' ganglion, centre, or knot of cells—that is, from cells which stand in immediate relation with ingoing fibres." The connecting fibres which I term "internuncial" *loc. cit.*, p. 586). Schiff speaks of as "kinesodic."

(*a*) The notion that we have in the sense of movement to do with a so-called "active sense," differing altogether in kind from other modes of sensibility ("passive senses"), seeing that its impressions are wrongly imagined to be "concomitants of the outgoing current."

(*b*) The notion that "mental operations in the last analysis must be merely the subjective side of sensory and motor substrata";[1] that we have such things as "motor ideas"; that "movement and sensation are the stuff of which our mental life is composed"; or that "at the root of our mental life, everywhere and always, there are movements."[2] If the activities of real motor centres are simple physiological processes (see p. 48) devoid in themselves of all psychical accompaniments, then the first phrase is an altogether erroneous one; while the subsequent phrases or statements are equally misleading, since they all imply that motor centres have such psychical accompaniments, and moreover confound them with the only real ideas of movement which we possess, viz., revived kinæsthetic impressions. As the writer has elsewhere said[3]:—"If the various impressions which go to make up the kinæsthetic sense are all of them (as we suppose) real 'ingoing' impressions that traverse different kinds of sensory nerves, the mere difference of the mode or occasion on which they are excited should not lead to them being spoken of as though they were radically different in nature from other sensory impressions. So that in accordance with this view, the dictum '*nihil est in intellectu, quod non fuerit prius in sensu*,' loses none of its old force."

It must not be supposed, moreover, that the evil consequences of the above-mentioned erroneous notions are limited to the spheres either of physiology or psychology. They are far-reaching in their effects. The associated doctrine, that words are revived in thought as "motor processes," tends, in my opinion, to throw confusion into the sphere of practical medicine by hampering the proper comprehension of the

[1] Hughlings-Jackson, in 'Clinical and Physiological Researches on the Nervous System' (reprint), 1876, pp. xx.-xxxvii.

[2] Ribot, in "Les mouvements et leur importance psychologique" ('Revue philosophique,' Dec. 1879).

[3] 'The Brain as an Organ of Mind,' 1880, p. 597.

great class of speech defects, and thereby, as I have attempted to show elsewhere, tends to check the advance of our knowledge of cerebral localisation in that direction.[1]

For all the reasons which have been above set forth, therefore, it seems to me to be a matter of extreme importance to recognize, that the excitable areas in the Rolandic and marginal regions of the cortex are in no proper sense of the term "motor centres," and that the evidence at present in our possession makes it extremely probable that they are termini for kinæsthetic impressions derived from muscles, so that their excitation in this or that region is the immediate precursor of this or that kind of voluntary movement.

DISCUSSION.

Dr. Ferrier.

Dr. Bastian has gone over much ground where it is unnecessary for me to follow him. I am, however, glad to be able to say, that in many points I am thoroughly in agreement with him, and in particular in all that relates to the sense of movement being dependent on centripetal impressions, and not on "outgoing" currents as contended for by Bain, Wundt, and others. I also admit that in the first edition of my work on the 'Functions of the Brain,' I used expressions in reference to the subjective aspect of motor substrata which appeared inconsistent with the views I had elsewhere enunciated on this topic; but I have made no change of front, and am glad that in the second edition of my work I have succeeded in clearing away any misconception that may have existed as to my real meaning, which is, that I consider that the activity of motor centres and motor nerves by themselves is entirely outside the sphere of consciousness. Their activity is revealed in consciousness only through the coincident functioning of sensory nerves and centres. But I must join issue with Dr. Bastian on almost everything else that he has said in reference to the nature and cortical localization of the "so-called" muscular sense. I use the expression "so-called," because I think it is a misleading term. What is merely a complex assemblage of impressions of different categories, has no claim to be regarded as a

[1] See p. 50.

sixth sense, even though we admit in the fullest manner that the sensations connected with movement and muscular strain greatly extend the range of our appreciation of weights and resistances, beyond that founded on mere cutaneous pressure.

I should prefer the term "sense of movement," or even adopt Dr. Bastian's term "kinæsthesis," if I might do so without adopting the meaning he attaches to it. The sense of movement comprises impressions derived from the skin, fasciæ, tendons, joints and muscles themselves, conditioned by the act of movement. But the muscular fibre itself is of little or no moment in this relation; for the sense of movement and degree of strain passively communicated to a limb, may remain, to all intents and purposes, unimpaired, when the muscular fibre, motor nerves, and anterior horns of the spinal cord are entirely degenerated, as may be easily demonstrated in any incurable case of anterior poliomyelitis.

I entirely demur to inclusion in the muscular sense any "unfelt," or, as Dr. Bastian now prefers to say, "little felt" impressions. The sense of movement is an act of conscious discrimination, and I am unable to understand how unfelt impressions can affect the sum of this consciousness. Unfelt impressions are necessary for co-ordination of movements, but co-ordination of movement and sense of movement are two totally distinct things. As a rule, the sense of movement accompanies all movements, but we may have defects of co-ordination without defects in muscular sense, and loss of muscular sense without inco-ordination.

Illustrations of the first of these conditions are furnished plentifully by cases of locomotor ataxy, in which the sense of movement is retained in its integrity notwithstanding the most profound disorders of co-ordination. Of the second condition we have a crucial instance in the well-known case of Remigius Lens, quoted by Dr. Bastian (p. 14). Here we have a case in which, notwithstanding absolute cutaneous anæsthesia and loss of all sense of movement, the individual was able, with the aid of vision, to co-ordinate his movements with perfect precision, and even without the aid of vision, to use his hands with a very fair degree of steadiness. "If he is asked to raise his foot to a given height while his eyes are shut, he accomplishes the act by a perfectly quiet and suitable motion" (p. 15).

Dr. Bastian regards this as an exceptional case. In one sense it is so, but it is to my mind a crucial experiment proving that co-ordination is possible in the entire absence of any sense of movement. That in general the removal of the sensations accompanying muscular action should for a time lead to marked

uncertainty of movement is only what we should expect; but the point of this case is the demonstration, that they are not essential to accurate co-ordination, and may be entirely compensated for by visual sensations and visual ideas of movement. Again, in hemianæsthesia, whether functional or organic, there may be complete abolition of all kinds of sensibility, cutaneous as well as muscular, with complete retention of motor co-ordination. The movements of the hemianæsthetic are not inco-ordinate or ataxic.

I confess I am amazed at Dr. Bastian's statement (p. 19), that in hemianæsthesia "there is generally no very appreciable loss of 'muscular sense.'" I have myself examined several cases of this kind, and I have read of many more, and I do not know a single observer, with the exception of Dr. Bastian, who has ever seen a case of hemianæsthesia in which muscular sense was not abolished, or impaired in proportion to the degree of the anæsthesia existent. If Dr. Bastian is right in regarding mere co-ordination of movement as proof of the existence of muscular sense, then we might admit his conclusion. But his argument, in face of the facts mentioned, is a manifest *petitio principii*. I have elsewhere stated[1] that defects in muscular sense are usually associated with defects in common or tactile sensibility, and that I am not aware of any case in which there has been loss or impairment of muscular sense without coincident impairment of tactile sensibility. That it may not occur I will not deny, but I deny that it has been satisfactorily proved. It is stated by Brown-Séquard, and generally accepted, that there is loss of muscular sense on the limb on the side of hemisection of the spinal cord, and retention of this sense on the other limb which is otherwise entirely devoid of sensibility. I find that the only ground for this assertion is the fact, that the anæsthetic limb is directed with fair precision in its movements. On this fact the hypothesis is based, that the muscular sense is retained, because it is believed that the power of directing movements is dependent on the muscular sense—a conclusion which I hold to be entirely without foundation. The power of directing movements has no necessary connection with the muscular sense, and may be perfectly well retained when there is not a vestige of any sense of movement.[2]

In proof of the retention of muscular sense, in spite of profound impairment of tactile or common sensibility, Dr. Bastian refers to some observations by Leyden and Bernhardt. Leyden[3] records two cases of ataxy, with great loss of tactile sensibility, in

[1] 'The Functions of the Brain,' second edition, p. 64.

[2] See "Hemisection of the Spinal Cord." 'Brain,' April 1884.

[3] Virchow's 'Archiv,' Bd. 47.

which nevertheless the power of appreciating differences in weight was not lost. But he makes the very important statement, that these patients could only discriminate between *heavy* weights. And I have elsewhere endeavoured to show,[1] that the power of discrimination in these cases was probably dependent on the degree of general bodily strain, altogether apart from the sensibility of the limbs themselves to which the heavy weights were attached. Bernhardt's statements do not differ from those of Leyden. Such cases lend little support to Dr. Bastian's views. In reference to the next statement, that muscular sense may be lost in presence of normal tactile or common sensibility, I must say that I have myself never seen such a condition. Nor has Leyden. In investigating the tactile sensibility of a limb, I never consider that this is normal unless the slightest touch is clearly perceived and accurately localised. Unless this is distinctly proved and recorded, I refuse to accept any mere assertion, that the tactile sensibility was normal in any case. A prick of a pin may be felt, or a pinch, or pressure, when there is profound impairment of tactile sensibility and localisation of impressions, notwithstanding. And considering the great carelessness which characterises clinical records in the investigation of tactile sensibility, I decline to accept statements as to tactile sensibility being normal, unless we have good reason for believing that it has been satisfactorily investigated.

Yet I find that Dr. Bastian, with all his experience, is able to adduce only two cases, both hysterical, in support of his assertion, that muscular sense may be lost apart from affection of tactile sensibility. In the first of these cases (p. 23) sensibility was said to have been absolutely intact. In the second (p. 24) "sensation of contact and of temperature preserved; sensations to pain obtuse and abolished in places." The second case may therefore be eliminated, and all that remains is the first.

For the reasons above stated, I decline to accept this case without further confirmation. As there was no post-mortem examination in Zenner's case of supposed injury to the Rolandic area alluded to by Dr. Bastian (p. 25), it is useless to argue about the position and extent of the lesion, though it certainly confirms my statement of the intimate relation between cutaneous and muscular sensibility. "Cutaneous sensibility is good everywhere excepting over the fingers. . . . *The knowledge of the position of the fingers is impaired.*" The knowledge of the position of the other parts was presumably normal, notwithstanding their paretic condition.

[1] The 'Functions of the Brain,' second edition, p. 392.

As to the cortical localisation of the so-called muscular sense, I hold that the centre for this and for all forms of tactile and common sensibility is the falciform lobe; and that there is no loss or impairment of tactile or muscular sensibility except in connection with direct or indirect affection of this region, or of the sensory tracts of the internal capsule. The motor centres are distinct from the sensory as much in the cerebral cortex as in the spinal cord. The evidence in favour of the motor character of the Rolandic zone is precisely of the same nature as that in favour of all other centres and tracts termed motor. Excitation causes movement, and destruction causes paralysis of movement, *and of movement only*. Dr. Bastian objects to the evidence on this head drawn from experiments on the lower animals only. I myself have always insisted on the importance of clinical investigation in all matters relating to subjectivity, and I am quite content to take Bastian on clinical ground alone. I have seen cases of cortical lesions of the motor zone—cases of aphasia—in which the individuals have been able to indicate with the left arm every position passively communicated to their paralysed right.

Dr. Bastian has attempted—with, in my opinion, poor success—to extract support of his views from the cases of operation for cerebral tumour, which we have recently had in Queen Square. In the case of James B. (p. 27), who was under my care, there was no affection of sensation, though a decided degree of right hemiplegia, before the operation. The operation was of such a nature as to involve the gyrus fornicatus or its tracts. Hence the slight amount of impairment of tactile sensibility, *and with it of the sense of position of the fingers.*

In Dr. Jackson's case of Thomas W. (p. 29) there was still clearer evidence of injury of the sensory tracts. For after the removal of the tumour from the right parietal region, which resulted in paralysis of the left arm and slight paralysis of the face, there was *tactile anæsthesia over the whole of the left side of the body.* Unless we are to suppose, therefore, that the motor centre of the arm is not merely the "kinæsthetic" centre of the arm, but also the tactile centre of the whole of that side of the body, we may say that beyond all doubt in this case the sensory tracts of the internal capsule were injured, which they might readily be from the nature of the disease, and the deep incision necessary to remove it.

But what will Dr. Bastian say to the following case, at present under my care in the hospital? It is the case of J. H., who in January of this year began to suffer from left-sided convulsions, followed by hemiplegia, dependent on cerebral tumour affecting the Rolandic zone of the right hemisphere. At the time of ad-

mission into the hospital on Sept. 22, he was usually in a semi-comatose condition, but occasionally he woke up and was able to answer questions. From the notes of his case in the clinical record I extract the following:—

"*Movement.*—Cannot move the left arm or leg at all. Face-paralysis almost complete, at lower part on left side. Angle of mouth moves a little when he tries to show his teeth. Can close and screw up his left eye well. Tongue protruded well, but deviates to left side.

"*Sensation.*—Normal. Patient feels a touch with a quill readily over both sides, and can describe where the stimulus is applied. Painful sensation also normal. Was able to localise sensation readily and accurately. Muscular sense also normal."

On September 23rd, Mr. Horsley, at my request, removed the tumour. This had a diameter of nearly three inches, and occupied the middle of the Rolandic zone and invaded the cortex, and subjacent medullary fibres, to a considerable depth.

On September 24th, the clinical record reports as follows:

"Patient quiet, perfectly conscious. . . .

"Left leg and arm remain completely paralysed; face is also paralysed, but not quite completely. Sensation practically normal over whole of left side; though sometimes patient refers a touch to the nearest joint above the part stimulated, *e.g.* when the hand is touched, he refers to the wrist; when the forearm, to the elbow. This is not always so, as sometimes he points to or localises quite correctly the part touched. Muscular sense good; he can describe fairly well the position of his limbs.

"*Sept. 25th.*—Condition of sensation and reflexes the same as yesterday.

"*Sept. 26th.*—Condition much as yesterday.

"*Sept. 27th.*—Sensation slightly defective on face and arm or left side, and perhaps over leg, *i.e.* that patient generally, but not always, feels a light touch with the feather of a quill, and can often localise it correctly; but sometimes refers it to some other spot near, as when touched on the back of a finger, he thought it was the back of his hand. Muscular sense normal.

"*Sept. 28th.*—Always feels a touch, but does not localise it correctly. Touches on all fingers of left hand he refers to the thumb; on the back of the hand to wrist. But his answers vary, and sometimes he appears to localise correctly. On face and leg the localisation is usually accurate. Muscular sense perfect as to position, but when a finger is bent, although he says it is so, he often does not recognize which finger it is.

"*Sept. 29th.*—Refers touch on fingers to the thumb, as he did

yesterday. On face, sensation not quite as accurate as on right side. Sometimes he does not perceive a very light touch with the feather of a quill. Over leg and foot sensation is normal. Muscular sense normal."

After this date the patient began to experience some pain and tension in the region of the wound, and a considerable swelling ensued.

On October 6th sensation, cutaneous and muscular, was found to be considerably impaired over the whole of the left side.

The patient recovered so as to be able to leave the hospital; hemiplegic on the left side, totally as regards the arm, but partially only as regards the leg. Some degree of anæsthesia to light contact, and impaired localisation specially on arm and hand remained.

The facts of this case are of the utmost importance in reference to the question before us. Here we have a case demonstrably of cortical and subcortical disease in which, with the most complete paralysis of motion, sensation, cutaneous and muscular, was perfectly normal. Even after removal of the disease, at a time when any positive disturbances of sensation might readily have been ascribed to general perturbation of the brain, and not to the local lesion, we have for several days practically perfect retention of cutaneous and muscular sense.

It is true that, later on, coincident with certain morbid changes which showed themselves in the wound, and which I at the time ascribed to further extension (since verified) of the original lesion, a considerable degree of anæsthesia manifested itself on the left side. But this might well be from implication of the sensory tracts of the internal capsule; and the fact remains, that an enormous amount of the motor area of the right hemisphere was destroyed without in any appreciable degree impairing the sense of movement of the limbs entirely paralysed as to motion. This I hold to be a crucial experiment, which absolutely knocks the ground from under Dr. Bastian's feet.

The facts which I have brought forward prove, that the motor centres are motor centres in precisely the same sense as other motor centres. Excitation causes movement, destruction causes paralysis of movement, and of this only. Further, when these centres are destroyed, degeneration proceeds in the pyramidal tracts, precisely as it does in the motor nerves when the anterior horns are destroyed. If Dr. Bastian's theory of these centres is correct—that they are only excito-motor of true motor centres in the corpus striatum or spinal cord—then the pyramidal tracts must be merely afferent or intracentral. But inasmuch as he

body, is asked to move his arm, he makes an effort to comply with the request. He *knows* that he has made the effort, and *believes* that this effort has been effective in moving the left arm in a particular direction. If, however, the patient's eyes be closed he has no means of verifying his belief, but he still knows that he has made the effort. But, as has been first pointed out by Dr. Ferrier, the sense of effort consists chiefly of the feelings which accompany that closure of the glottis and arrest of respiration which is necessary to give fixity to the muscles which find their origin in the bones of the trunk. Now the muscles of respiration are bilaterally associated, and the activity of the one hemisphere—the right in the supposed case—sets in motion the muscles of both sides, and the sense of having made an effort would be awakened by currents passing inwards along the centripetal nerves of the right or healthy respiratory muscles and of the right side of the glottis. That this theory affords a better explanation of the facts than the one which seems to be favoured by Mr. Sully, viz. that the sense of effort is correlated with the out-going stream, is much strengthened by the case of hysterical anæsthesia quoted by Dr. Bastian on page 16, from Duchenne and Briquet. "If," say the authors, "after having made her squeeze the hand, one prevents her seeing it, and tells her to cease all effort, one feels that the contraction of the flexors continues, and it is necessary to employ considerable force to open her hand." It will be generally admitted that the persistent contraction of the flexors in this experiment was caused by a continuous outflow of energy from the cortex, and yet the patient remained quite unconscious of making any kind of muscular effort.

Most of the questions under discussion to-night are questions of interpretation and not of fact, but there is one issue between Dr. Bastian and Dr. Ferrier which is one of fact and not of interpretation. Dr. Bastian says that in hysterical hemianæsthesia the muscular sense is almost always preserved, while Dr. Ferrier asserts strongly that it is lost. I must myself plead guilty to having stated, in more than one publication, that in such cases the muscular sense is lost, but I have in recent years observed many cases in which it was preserved. A few years ago I had a patient under me who had had an attack of left-sided hemiplegia, in which the leg was more paralysed than the arm, and as usual in such cases the motor paralysis was accompanied by hemianæsthesia. In this case it may be presumed that the symptoms were caused by a lesion, probably a hæmorrhagic focus in the lenticular nucleus and implicating the posterior part of the internal capsule. When a book was placed in this patient's

expressly admits that they are efferent and convey motor impulses, he appears to me to admit all that is characteristic of the activity of motor centres. Just as the activity of the anterior horns of the severed spinal cord is displayed in response to stimuli proceeding from the posterior roots and horns, so the motor centres of the brain are thrown into activity by stimuli proceeding from the sensory centres—visual, auditory, tactile, &c. As the motor nerves convey stimuli to the contractile muscular fibres, so the pyramidal tracts convey impulses to the motor apparatus—not perhaps directly to the muscular fibre—but to the neuro-muscular mechanism by which actions are capable of being effected. As I have elsewhere ('The Functions of the Brain') discussed at some length the mechanism of volitional movements, the basis of our ideas of movement, and the relations of the motor centres to these and to motor acquisitions, I do not consider it necessary to discuss these questions further on the present occasion.

Mr. Sully.

Mr. Sully remarked that though he felt the force of the facts and arguments in favour of the view, that the muscular sense was, on its physiological side, essentially afferent, he could not accept this as a complete theory of the subject. Among other facts which seemed to conflict with this view were the hallucinations that were known to follow the loss of a limb. When in these circumstances a patient was able to give a certain degree of definiteness to his feeling of the (imagined) situation, and movements of the missing member, this appeared to point to the co-operation of the efferent nerve-process as a factor in the muscular sense. It is not unlikely that the so-called muscular sensations are highly complex psychical phenomena, in which afferent currents play a prominent part in giving the exact measure of direction, rapidity, and distance of movement, though an essential, and perhaps the most characteristic element, is supplied by the efferent process of innervation itself.

Dr. Ross.

Before entering on the general discussion, I shall endeavour in a few words to explain the difficulty started by Mr. Sully, so as to bring it into accord with the theory, that the sense of muscular movement is correlated with the activity of in-going and not of out-going currents. When a person suffering from a profound degree of hemianæsthesia, say of the left half of the

hand she held it quite well so long as her eyes were fixed upon it, but on her attention being diverted by getting her to put out her tongue, the grasp immediately relaxed and the book fell, but the patient continued to hold out her hand horizontally as if she were still supporting the book. In her case the muscular sense was lost. I have tried this simple experiment in many cases of hysterical hemianæsthesia since this patient was under observation, but have not met with one who dropped a book or other weight from the anæsthetic hand on the eyes being closed. It seems to me, therefore, that in most cases of hysterical hemianæsthesia the muscular sense is retained, although I have no doubt that it may be lost in aggravated cases. But if the muscular sense is lost in any case of hemianæsthesia without this loss being accompanied by a corresponding degree of motor paralysis, the argument which Dr. Ferrier has founded upon the fact is as valid as if this sense were lost in all cases of the kind. In carrying on a discussion upon any controverted subject, the upholders of a certain theory ought to adduce arguments the validity of which will be acknowledged by their opponents, and it must be acknowledged that although Dr. Ferrier's argument carries conviction to my mind and to the minds of those who accept his theory of psycho-motor action, yet it is not likely to be equally convincing to those who reject his theory. It is still open to Dr. Bastian to reply, and indeed he does reply, that although it be granted that the muscular sense is sometimes lost in the absence of motor paralysis, yet this loss is caused by injury of the centripetal conducting paths and not by destruction of the kinæsthetic centre itself. When the centre itself is destroyed, it causes, according to Dr. Bastian's theory, an equal degree of motor paralysis and of loss of the muscular sense. I repeat that I do not agree with Dr. Bastian, but I fear no evidence which I can adduce in reference to this point will suffice to drive Dr. Bastian from the position which he has occupied, and compel him to agree with our views.

It is a trite remark, at least since the days of Locke, that most of men's disputations arise from the use of ambiguous terms, and certainly the terminology adopted by Dr. Bastian seems to me to favour every species of fallacy and confusion. Take, for example, his nomenclature of animal movements, which he divides into five classes, viz. (1) reflex or automatic, (2) secondary-automatic, (3) instinctive, (4) ideo-motor, and (5) volitional. In this classification, reflex and automatic are used as synonyms for the same kind of movement. Here, then, there is a merging together of two valuable terms, one of which is well adapted to express the kind of movement indicated, while the other ought to be reserved

for some other kind of movement. It is possible that no great confusion might result if these words were always consistently used as synonyms for the same kind of action, but they are not. The element of confusion arises chiefly through the use of the term *secondary-automatic*. Dr. Bastian argues that voluntary movements tend by frequent repetition to become first ideo-motor and then secondary-automatic, but whether or not he makes any distinction even of degree between the movements indicated by the two last terms is not very clear. Now if a voluntary movement may by frequent repetition become secondary automatic, and if automatic and reflex are merely synonymous terms for the same kind of movement, it is clear that, in his opinion, a voluntary movement may by frequent repetition, become reflex. This view I should most strenuously oppose. I do not believe that any amount of repetition tends to convert a movement which was once voluntary into a reflex action either in the individual or the race. But the confusion which arises from the improper use of such terms as reflex, automatic, and secondary-automatic is small and unimportant as compared with that which must follow the indiscriminate use of such terms as instinctive motor, ideo-motor, volitional motor, and other compounds of metaphysical and physical ideas of the same class. Surely the psychology of the present day has outgrown the employment of such unscientific terms as these.

It seems to me that the term "impressive" is also used by Dr. Bastian in a very unguarded manner. He employs it at times to indicate the physical changes set up in the peripheral ends of certain nerves by muscular contractions, and at another time as a name for the sensations which accompany the movements caused by muscular contraction. This is by no means an unimportant objection. It is by a similar ambiguous use of the word "sensibility" that the late Mr. G. H. Lewes came to the conclusion, that the spinal cord possesses consciousness. The term sensibility was used at one time by Mr. Lewes as a generic name for the elementary feelings of the mind, and at another time as synonymous with nerve irritability. He found no difficulty in proving that the spinal cord is possessed of sensibility in the sense of nerve irritability, and from this he inferred that it possesses sensibility in the sense of feeling, from which the conclusion inevitably followed that the spinal cord is endowed with consciousness. The example of Mr. Lewes shows that, in passing from the molecular movement of nerve centres to the phenomena of the mind, it behoves psychologists to tread with cautious steps, but I have no hesitation in saying that Dr. Bastian's tread in this domain is not characterised by that care and caution which his eminence as

a writer and thinker on psychological subjects might lead us to expect.

Instead of the complex classification of animal movements adopted by Dr. Bastian, I should propose to divide them into reflex, psychical, and automatic, this division to be made simply from the character of the movements themselves, and without reference to the nature of the neuro-muscular mechanisms by which the movements are regulated and produced. A *reflex* action is an adjustment of the body to a *present* impression. If the palm of the hand of a person in profound sleep be touched by a feather, the fingers close over the feather, and if the sole of the foot is so touched it is drawn away; but in both examples the adjustment is to a present impression. A *psychical* action is one in which a present impression calls forth an adjustment of the body to an anticipated impression. If the word anticipated be objected to as implying consciousness which is not an accompaniment of every psychical action, it may be said that the present impression evokes an adjustment to a future impression. Of psychical actions there are two chief varieties: the one is general, or frequently repeated in the experience of the race and individual, and it is effected in the absence of consciousness; the other is special, or rarely repeated in the experience of the race and the individual, and is accompanied by the highest degree of consciousness called attention. Between the most general and the most special psychical actions there is every degree of generality, and these different degrees are accompanied by corresponding degrees of consciousness. It also follows that actions which are special and attended by a high degree of consciousness in certain races and in children become, by frequent repetition, general in other races and in adults, and are then attended by little or no consciousness. As an example of a psychical action I may adduce my own action in stretching out my hand to grasp that book on the table. A patch of colour reflected from the book falls on the back of my eye—that is the present impression. I stretch out my hand, not to reach the patch of colour or the present impression, but fully expecting that I shall experience certain tactual feelings, and feelings of resistance and weight—these being the anticipated or future impressions. An *automatic* action is exceedingly difficult to define. Taking the literal meaning of the word, it ought to imply an action which is self-evolved or spontaneous. Some physiologists believe that the respiratory movements are regulated by a centre in the medulla oblongata, which by its own spontaneous activity sends out rhythmical impulses to the muscles without itself being influenced by impulses received through centripetal nerves. Did such an action

as this exist it would be justly entitled to be called automatic, but the balance of evidence seems now to turn in favour of the opinion that the respiratory movements are regulated by a reflex mechanism, and physiologists doubt whether an automatic movement in the narrow sense just indicated anywhere exists in the body. The term automatic is often used to designate a self-adjusting mechanism, but every action of the body results from such a mechanism. It must, however, be admitted that the actions which I have named special psychical actions are regulated by nervous mechanisms which are only imperfectly organised, while the general psychical actions are regulated by fully organised nervous mechanisms, and the term automatic is frequently applied, and not altogether unjustly, to the latter of these two. At the same time I much prefer to dispense with the word automatic in this connection, and to use the word general or unconscious psychical actions in contradistinction to the special or conscious psychical actions, and I should reserve the term automatic to another kind of action altogether. The delicate adjustment by which a certain attitude of the body is maintained in changing circumstances might very appropriately be named an automatic action. The maintenance of the erect attitude on board of a ship tossed by the waves is a good example of the kind of action under consideration. The delicate bodily adjustments which are necessary to maintain this attitude cannot be regarded as being either reflex or psychical in character, and consequently the first of these adjustments ought to be differentiated from the last two by having a distinct name assigned to it, and the word automatic is well adapted for the purpose. In any case I object in the most emphatic manner to the word automatic being used now as synonymous with reflex, and again as indicating an unconscious psychical action. Used in this loose manner it is hardly possible to imagine a word more calculated to introduce confusion into the intricate question which is before us for discussion to-night. Dr. Bastian may legitimately reject my suggested use of these words, and it is open for him to define his terms in his own way, if only he is afterwards consistent with his own definitions; but it is not open to him to use words in several senses without making any attempt at a strict limitation of them. If all of us here to-night would but come to an agreement with regard to the meaning of our terms it appears to me that the differences in our opinions would be found to be exceedingly slender, and such as might be put into the proverbial nutshell.

Every one will admit that the nervous mechanisms which regulate psychical actions must possess centrifugal as well as

centripetal conducting paths, and centres which are in relation with the former as well as centres in relation with the latter. The main question which divides us, or at any rate which divides Dr. Bastian from Dr. Ferrier, is the point at which the centripetal conducting paths and centres cease and the centrifugal centres and paths begin. Most of us will at once say that this point is to be found in the large caudate pyramidal cells of the third layer up the cortex. Now if Dr. Bastian says that he will not accept this view it may be difficult to find an argument that will convince him. I will, however, address to him one argument which has great influence with myself, in the hope that it may have some effect upon him also. Dr. Bastian will admit that the molecular activity of the sensory centres is always correlated with feeling or states of consciousness, and he may possibly acknowledge that consciousness is correlated with the activity of cells and fibres which are not yet fully organised, or, as Dr. Hughlings-Jackson has expressed it, with the activity of centres which are "now organising." But the large pyramidal cells of the cortex, with their numerous connections and axis-cylinder process, are already fully organised, and I cannot imagine that the activity of these cells can be correlated in any degree with consciousness, and if not, these cells cannot be regarded as forming part of sensory centres. Whether this argument will or will not have any weight with Dr. Bastian I cannot tell, but I myself attach so much importance to it that were I compelled to abandon this position I should feel obliged to give up almost every idea I have formed of nervous action, and would have to begin the work of reconstruction over again. I think this argument ought to have considerable weight with our President, and were he to give it full consideration I believe it would force him to give up the theory, that the consciousness of muscular effort is correlated with the stream of out-going energy.

But although the presence of large pyramidal cells gives character to the parieto-frontal area of the cortex, yet it must be remembered that in the outer layers of the cortex in this area the cells are small and are either destitute of processes or the processes are only indefinitely connected with one another; in other words the cells are but partially organised, and consequently the activity of these cells is likely to be correlated with some form of feeling. It is therefore possible, I might even say probable, that the cortex of the parieto-frontal area possesses sensory as well as motor functions, and although we find it extremely convenient to speak of this area as the motor part of the cortex, yet this form of expression is more practically useful than theoretically just. I

feel myself wholly unable to pass any opinion upon the question, whether or not the cortical centres of muscular sense are more likely to be situated along with the tactile centres in the falciform lobule or in the superficial layers of the so-called motor area. I at least give my unhesitating adhesion to Dr. Bastian's opinion, when he says that the kinæsthetic centre is the last centre which must be excited anterior to the movement in any psychical action. The book which lies on the table there reflects certain rays of light on my eyes, but my mind instantly conceives the cause of the colour I perceive to be an object possessing certain dimensions and weight, and if I stretch out my hand to seize the book I do not expect to lay hold of the colour, but of something which will offer a certain resistance to my grasp, and require a certain effort to raise it from the table, and these ideas of resistance and effort are the correlatives of the activity of my kinæsthetic centres. The order of excitation in the psychical act of raising the book has therefore been visual conducting path and centre, kinæsthetic centre, motor centre conducting paths and muscles; while on the action being completed there is a final excitation of the connected kinæsthetic conducting paths and centre which is the correlative of the consciousness of the effort put forth in effecting the movement. Dr. Bastian would say that the kinæsthetic centre and the cortical motor centre are identical, but for the reasons already given I cannot assent to this proposition. I think it, however, quite likely that the kinæsthetic and motor centres coincide, in so far as that the former are situated in the two outer and the latter in the third layer of the cortical cells of the parieto-frontal area of the cortex. My mind is quite open to accept this opinion or the one which seems to be favoured by Dr. Ferrier, viz., that the kinæsthetic centres are situated along with the centres for all forms of cutaneous sensibility in the falciform lobule, and I await further experiments and observations to determine the question.

Sir James Crichton-Browne.

If Wundt's notion and that of Bain as to the sense of effort is to be abandoned, or if that sense is to be reduced from a feeling of the energising of motor centres to a feeling accompanying the reception of centripetal impressions generated by the act of muscular contraction, and especially by contraction of the respiratory muscles, I should be glad to have an explanation of the sense of fatigue, in consonance with the new hypothesis and covering ascertained facts. The sense of fatigue is certainly not

dependent on cutaneous sensibility, and it is something altogether different from the muscular sensibility experienced on galvanisation of the muscles. True, it may be argued, that the sense of fatigue is attributable to impressions made on sensory end-organs in the muscles, by carbonic and sarco-lactic acids and other waste products, which accumulate during exercise, but it is to be noted that it may be felt acutely, as in cases of neurasthenia, when the muscles are well nourished and have been long at rest, and when, consequently, they cannot be overcharged with metabolic *débris*. It has always seemed to me that the sense of fatigue, which is expressed, be it remembered, in an indisposition for mental as well as for bodily exertion, is a modification of the feeling of expended energy, dependent on an exhausted state of the motor centres akin to what we find after an epileptic fit. It has seemed to me that the opposite condition to fatigue, that of muscular restlessness or fidgets, the *besoin d'agir*, that keen appetite for muscular activity, which is revealed by caged wild animals when they pace up and down for hours together, which is seen in the exuberant activity of the young, and which all of us have experienced, more or less, after periods of repose or enforced idleness, is dependent on a surcharged or excitable state of the same centres. Again, of course it might be argued that fidgets are to be ascribed to sensory impressions derived from highly nourished and oxygenated muscles in a state of rest, but again I would point that we have these fidgets, or spontaneous outpourings of ebullient energy, in their most marked and persistent form, in the incipient stages of mania, when the muscles have already been over-used and are wasting rapidly, and so cannot be highly nourished or oxygenated or in a condition to originate a craving for further action which under such circumstances I should be disposed to trace to an erithiomic state of the cerebral motor-centres. The feeling of necessity for muscular exercise, which arises periodically or under special circumstances, seems to have no connexion with any antecedent sensation or emotion, or with any stimulus extraneous to the motor centres themselves. I am aware that other explanation of fatigue and fidgets may be advanced. The late Professor Laycock, for example, used to maintain, that they are due to the direct recognition by certain nerve centres of the varying composition of the blood circulating through them; but the explanation which seems best to cover all the facts and is altogether most feasible, is that which connects them with functional changes in the cerebral motor centres. But even if another explanation—a sensory one—of fatigue and fidgets were clearly established, I should not on that account give up the

sense of effort and motor ideas as opposed to ideas of movement, for I cannot divest myself of the belief that as we have perception at the end of the ingoing currents, we must have apprehension at the beginning of the outgoing currents. Perception is not the "be all and end all," and to relegate our whole thought-material to sensory centres, for that is what it comes to, according to Ferrier's most recent theory—is to degrade a large region of the cerebrum from its high estate, and leave it a mere superfluous intrusion in the brain mass. Professor James of Harvard, who is quoted by Ferrier and Bastian, has analysed a voluntary act or movement into four elements. 1. The preliminary idea of the end. 2. The fiat. 3. The muscular contraction. 4. The end felt as actually accomplished. Now of these four elements only one, the fiat, can, according to Professor James and those who think with him, be in any way connected with or dependent on cerebral motor centres. The preliminary idea is sensory, the contraction is muscular, and the recognition of the effect of the contraction is sensory. But the fiat, if it be further analysed, resolves itself very much into a balance of motives or the balance between two groups of sensory impressions, and all that is left for these large and important motor centres in the brain, with their special structure, is a mere motor impulse. But a motor impulse can be got from a reflex centre in the cord, and it would appear therefore that the large area of the hemispheres corresponding with the motor centres, is—if it has no part to play in ideation—redundant or superfluous, or has at least a very humble office to perform. On principles of physiological economy I cannot accept this. A more minute analysis of a voluntary act or movement than that offered by Professor James is, I think, necessary. We must analyse the preliminary idea and find in that sensory and motor factors, a reproduction of ideal movements as well as of ideal sensations. We must analyse the fiat and discover there not merely a haphazard motor impulse, but a nice adjustment of means to the end to be attained. We must analyse the contraction, and distinguish in it the latent period, shortening relaxation, electrical, chemical, and thermal changes. We must analyse the end felt as accomplished, and recognise in it an appreciation of the centric changes which have succeeded to the voluntary movements, as well as of peripheral changes in joints, skin and muscles. And then, having completed our analysis, we must synthetically recombine all our elements and lift them into the higher medium of the unity of consciousness. If I might be permitted to do so, I would take my friend Professor Ferrier, engaged in the performance of one of his own experiments, as a rough illustration or rather as a working model of a voluntary act.

He stands with the electrodes in his hands, and the anæsthetised monkey with its motor centres, or rather with its Rolandic area exposed, lies on the table before him. With that power of prediction which is the true test alike of science and volition, he says, "I will cause the animal to clench its right fist." That is the preliminary idea, and if we examine the form of words in which it is embodied, we shall be able to satisfy ourselves that it is a complex proposition, involving, like every predicate, sensory and motor ideas—the revival, not only of impressions, but of processes, implying not only observation but the knowledge of power Well, what is the next step that the Professor takes? Why, he applies the electrodes to the tip of his tongue, to ascertain that the current is neither too strong nor too weak, and he shifts the induction coil according to what experience has taught him to be necessary to secure the end in view, and in doing this he represents dramatically, and on the large scale, a factor in a voluntary act which Professor James has ignored—that nice adjustment of and co-ordination of the means used to the end to be attained, that regulation of the jet—without which all voluntary movements would be ataxic or disproportionate or tentative. In the initiation of a voluntary movement sensory impressions from the part to be moved can play no part. They come a stage too late to supply any guidance in determining the amount or intensity of the outgoing current, and unless feeling accompanying it regulates that, it is difficult to understand how it can ever be anything but experimental in the first instance. After regulating his current, Ferrier applies the adjusted electrodes to a point on the left ascending parietal convolution—that is the fiat—the essence it may be said of the voluntary act, and in executing it he sees and moves, and selects in the latter case performing what must be in itself a complex operation. The fiat given, the right fist is clenched; that is the muscular contraction, and after that were the animal not anæsthetised, the effects of that contraction would be transmitted to the brain for further guidance, or for registration there, along with the effects of the discharge in the centres themselves. At one with Ferrier as I am in regarding the Rolandic area of the brain as made up of true motor and not of kinæsthetic centres, I cannot follow him in depriving these centres of their ideational function.

Sir James concluded by congratulating Dr. Bastian on the grasp, subtlety and lucidity of his paper, which would be heartily acknowledged, he said, by those who differed from him most.

DR. HUGHLINGS-JACKSON.

The question seems to me to be an exceedingly large one, and to come to a great extent to this: Do motor elements enter into the physical bases, or anatomical substrata, of mental states, otherwise consciousness? I have for many years thought that they do. Since it is agreed upon that the lower parts of the nervous system are sensori-motor, I think it *à priori* likely that the higher parts, the physical bases of consciousness, are so too. The unit of constitution of the nervous system throughout is a sensori-motor unit. That would seem to me to agree with the doctrine of Evolution.

We have, as medical men, this problem:—Epilepsy being owing to disease of the cerebral cortex, and insanity being owing to a different kind of disease of the cerebral cortex, what is the constitution of "the organ of mind," the cerebrum, whereby two such different things result from disease of it, having regard of course to the distinction between the physical and the psychical? Moreover, we have the two morbid states in sequence often enough. A man during an epileptic fit, if the primary discharge be strong enough, is stiffened into a rigid mass; after the fit he is temporarily insane. Further we have to show what are the anatomical and physiological conditions of the nervous system correlative with will, memory, reason, and emotion (together states of object consciousness). They are, I submit, activities of sensori-motor nervous arrangements—of nervous arrangements representing impressions and movements of parts of the body. We have also to find the physical differences answering to the differences between faint and vivid states of consciousness (object-consciousness). I submit that, neglecting differences in the sensory sphere, since no one denies the sensory element, the former occur during slight activities limited to the highest motor centres, and the latter during stronger activities of those centres overcoming lower motor centres and gaining the periphery. To my thinking, not only are the convolutions in the Rolandic region motor (middle motor centres) but also the convolutions in front, frontal or præfrontal lobes (highest motor centres). The physical process during ideation differs from that during perception (both being states of object consciousness), in that in the former lower motor centres are not overcome, and in the latter are.

There is no *à priori* reason why states of consciousness should not attend activities of motor as well as activities of sensory cells. It may seem easier to believe that sensations attend activities of sensory elements than that they attend activities of motor

elements. The popular psychologist makes a confusion betwixt active states of sensory elements (physical states) and sensations (psychical states). Starting with what is purely physical, active states of sensory nerves, we get at length, in nervous centres, unconscious sensations (a contradictory expression) and then conscious sensations (a tautological expression). These sensations are then compounded in the highest centres into ideas. After that the popular psychologist inverts the process. Now ideas, sensations, and volitions (psychical states) produce movements (physical states); there is the expression "ideo-motor." In reality, movements result from liberations of energy, and not from states of consciousness.

The common expression "ideas of movements" is very objectionable. I would submit that there is no more an idea of a movement (in the sense either of ideas of states of muscles or of any motor or sensory nervous elements representing any combination of muscles) than that, when I think of, or see a colour (coloured object), there is "an idea of" the retina or of any sensory centres representing it. I submit that we have psychical states attending activities of motor elements of the highest centres. For these psychical states there is no name, except in the case of words; words are psychical states attending activities of motor elements representing certain very special and complex articulatory movements. (Strictly the nervous process is always sensori-motor.) I dwell on the motor element, no one denying the sensory element. I contend that when I make a movement with my hand I have a psychical state just as certainly as I have another when I see that red chair. For if I cut my arm off I have a spectral hand remaining; that spectral hand I can move. This (certainly a psychical state although one uses the term "move") attends, I submit, activities of motor elements of my highest centres. When I cut my hand off I have a ghost of it, but of course that ghost was there when my arm was present. When a man who has lost a hand tries to pick up the reins of his horse with his ghostly hand, that "movement" of it occurs during energising, beginning in the highest motor centres. Dr. Bastian would say that it occurred during activity of kinæsthetic centres. I admit, and am assuming, the activity of sensory elements, but I contend that there is activity of motor elements as well. I submit that the expressions "motor ideas," "ideas of movement," and "muscular sense" (if "muscular sensations" be intended) are illegitimate compounds of physiological and psychological terms.

I have often referred to the well-known displacements of external objects, in cases of ocular paralysis, in proof that psychical states

attend activities of motor elements; but I find that these very cases are claimed by those who repudiate the "out-going current doctrine." I suppose the alterations in size of an after-image during changes of accommodation will be similarly claimed. The following, which I used to think a very strong thing favouring the doctrine that mental states attend activities of motor elements in the highest centres—the alteration in shape of after images—will, I suppose, be claimed too. If we obtain an after-image of a square, and then project it on to an inclined sheet of paper, the spectral square becomes oblong. There is here a change in muscular activity, but it may be said that no mental states attend activities of the motor nervous arrangements effecting it, but only of the sensory elements in activity from changes in the muscles put in action. Let us take an extreme case. A man has progressive muscular atrophy to the degree that he has no muscles in the right arm, and no anterior horns for them. Yet he localises a touch, and for the sake of argument I will grant that he does so as well as ever. I suppose the localisation implies movement, in this case the so-called "ideas of movement." It is true that the patient has no muscles, but the whole of his sensory nervous system is intact, and the whole of his motor nervous system with the exception stated. My submission is that the patient localises the impression because, besides sensory activities, there are also activities of his highest motor centres.

PROFESSOR HORSLEY.

As some cases I have published have been introduced into this discussion, I shall be very glad of the opportunity to say something with respect to the representation of the "muscular sense" in the so-called motor region of the cortex. In the first place, let me say that if I use the terms usually employed, apparently with a twofold meaning, I hope I shall not be misunderstood. Thus if I speak of "sensation" I do not mean by that term, in every sense that I may use it, to connote consciousness. We are very much in want of a term to express better what is generally understood by the word sensation. The changes which go on in nerve endings when they are irritated require some other expression than our common word sensation, which conveys the meaning of the conscious perception of the process which is going on peripherally. If therefore I use the word "sensation," I hope to be understood that in a large majority of cases I shall be referring to a conscious perception of the changes in the nerve endings, but I may also mean the changes which go on in the nerve endings

without the accompanying consciousness. Further, a great deal of stress has been laid on the use of the word motor. What Dr. Ferrier calls "motor," Dr. Bastian, for equally good reasons, calls "efferent"; what Dr. Bastian calls "efferent," Prof. Schiff calls "kinesodic." Now it seems to me that of these the term efferent is the more general in meaning, therefore I shall, as far as possible, avoid speaking of "motor," and I shall apply the term afferent to the impulses which originate the so-called motor sense. There is no doubt as to what is meant by the word afferent. It seems to me that so much stress has been laid upon the meaning of these terms as to introduce no little confusion into this matter. Well, with respect to the first point, viz., the cases of my operations which apparently throw light on this question, personally I desire to lay no great weight on these cases. There are not enough of them yet for us to speak absolutely from the evidence they afford. There is among them one case of cerebral tumour, in which I did, as Dr. Ferrier says, make an incision deeply down into the corona radiata. To my mind the knife did not pass down deeper than one inch. Personally I do not consider that that incision injured the fibres going to the so-called sensory tracts of the cortex, but until the patient dies, I would not like to say exactly what was the extent of that lesion, and therefore, as I stated in the description I gave of my cases in the 'British Medical Journal' for Oct. 10, 1886, I would not rely on their evidence further than to regard them as tending to show that the so-called motor region is the centre of muscular sense. I would put aside that case which Dr. Ferrier has quoted of the man Henderson, for although there certainly seemed to be a delay on the onset of the anæsthesia after the operation, the whole surroundings of the case are so very complicated that even after the death of the patient I should not like to adduce it alone in support of an argument one way or the other. When one has a series of something like twenty-five or fifty cases, then I should like to come forward with some more positive statements. These cases however, to my mind, do tend to show there is possibly some representation of the so-called motor sense in the motor cortex. Lastly, we come to the question of the theoretical possibility, that the muscular sense is represented in the so-called motor cortex. It is not for me to enter to-night into questions of much deeper import, questions of philosophic thought. It would be presumptuous on my part, after what has been said before, by Drs. Bastian, Ferrier, Ross, and Sir J. Crichton-Browne; but as regards the anatomy, *i.e.* the mechanism of the function of which we are now discussing, I should like to say a few words. Dr. Ross has referred to the two main histological divisions of the

cortex, viz. the large ganglionic, the so-called motor cells of the fourth layer, and the smaller cells which exist in vast numbers in the rest of the grey cortex, and he believes that consciousness is correlated with the functions of the smaller cells, and not with the larger ones. I would venture to say that such a belief as that is of course a personal matter, but argument of analogy seems to me to be strongly in favour of this contention. But there is another mode of regarding the anatomical facts just mentioned, and that is the possibility of each being correlated to efferent and afferent functions respectively. Of course, theoretically, we may for many reasons regard the few large corpuscles as essentially efferent, and the small cells as essentially afferent in function. For myself, I am quite prepared to hear it proved some years later that the smaller cells have a distinctly sensory aspect, and that the larger cells have a distinctly motor aspect. Until this anatomical problem has been cleared up, we must leave the question entirely open whether the smaller cells are kinæsthetic or not. Dr. Ferrier would say absolutely that they are not; that there is no representation of sensation in this "motor" region, and that therefore the function of these smaller cells is motor. That the whole section of the brain cortex in this region is motor, it is very difficult to believe, though such a view is apparently supported by some very considerable facts. Finally, as regards experimental evidence, enough has been said on that point to avoid adding to it. The only experiments that will show anything are the observations on human beings. Until after small localised portions of that region have been excised, and a reliably large number of such cases have been thoroughly examined and reported, it will be impossible to do more on this problem than admit its present insolubility.

Professor Haycraft.

I should like to draw attention to a change which has recently taken place in the views of many physiologists respecting the functions of nerve cells and nerve centres. This change is due, I think, to a more philosophical conception of the development of the brain, and to the introduction of improved technical methods for its histological investigation. I venture to make these remarks, because in my opinion the discussion, as to whether certain regions of the cerebral cortex are motor or sensory, can resolve itself into a statement of what they really are in terms of these newer conceptions. We must remember that the central nervous system in higher animals is interposed between the sensory surfaces of the body and the muscular system, etc. and that the nervous connec-

tion between a sensory surface and a muscle forms a loop passing through the brain or cord. In its path this loop is beset with nerve cells, which are spoken of as being sensory or motor cells. As a result of stimulating the skin of an arm, an impulse will pass to the cord and may turn back to a muscle of the same limb. It may, however, make a larger circuit, and pass up to the brain, to the grey matter around the fissure of Rolando, back to the cord, and out to the muscle. In the second case, it passes to the brain, in order that it may be correlated with other impulses passing in other loops belonging to the special senses; for the brain is to be considered strictly as an enlargement due to the presence of these sense organs. There is a greater and greater tendency to look upon the central nervous system as nothing more than a vast mass of communications, whereby both sensory and muscular areas can be brought into relationship, whereby the simple connecting strands between sensory cell and muscle, isolated in the lower, are bound together in higher animals. The discoveries of Exner compel us to look upon that wonderful plexus of medullary fibres which he has demonstrated in the cortex, as more important even than the nerve cells found there. No one could examine a preparation of the cortex, prepared by Weigert's method, without being convinced that the interlacing fibres to be observed, connected ultimately with every muscle and sensory surface of the body, form a mechanism which will account for our possession both of muscular co-ordination and intellectual association. If at one time certain cells in this intercommunicating meshwork were looked upon as the special seat of sensation and others as the seat of motion, it is impossible so to view them now. As a result of the passage of an impulse through the nervous system, we may have muscular movement and we may have sensation, but in the nervous path through which the impulse passes it is not possible to say that one part is more motor than another, or to localise sensation to any given spot. Every portion of the path receives an impulse which originates in a sensory area, and passes ultimately to a muscle. Were we to define a sensory cell merely as one that receives an impulse from a sensory surface, and a motor cell as one that transmits an impulse directly to a muscle, we are at once met with a difficulty. The cells in the cortex are on a loop between the sensory and motor cells of the cord, and to which do they belong? It may be inquired, what are then the uses of these cells? It is probable they act as junctions in the nervous network, as suggested by Dr. Hill. They are trophic, and perhaps they act like collections of combustible material placed on a train of gunpowder. In any case it will be advisable to use the terms

sensory and motor quite tentatively, and preferably to speak of these so-called cortical centres as areas of the nervous system through which pass certain sensori-motor loops, and whose destruction will render impossible the productions of those muscular movements, etc. which had previously depended upon their existence.

DR. MERCIER.

Before entering on this discussion, may I first be permitted to express the sense of incongruity which it arouses? It appears to me that a discussion by the Neurological Society, as to whether or not the idea of movement is a concomitant of the outgoing current, bears a certain analogy to a discussion by the Astronomical Society as to whether or not the earth is round.

Dr. Bastian has complained that there has been shown no *à priori* reason why the generally received doctrine should be preferred to his own, which I with difficulty refrain from calling the geocentric doctrine of neurology. This is a complaint which he ought not to have the opportunity of making again, and for want of a better I will supply him with an argument which is, I hope, not open to objections on the score of deficiency in *à priori* character. The organism exists not alone in space, but in the midst of surroundings. Between it and its surroundings but two actions are possible, viz. action of the surroundings on the organism, and action of the organism on its surroundings. These two actions are of co-ordinate importance. Consciousness cannot exist without both. Life cannot exist without both. Into every manifestation of consciousness and into every manifestation of life both elements enter as necessary constituents. We may separate them in analysis, but in fact they are inseparable. In the constitution of consciousness and of life neither of these elements is before nor after the other, neither is greater nor less than the other. Yet, according to Dr. Bastian, the actions of the environment on the organism are known immediately and directly, while the actions of the organism on the environment are known only mediately and indirectly and subordinately and at second hand.

Much has been said as to whether this or that case has been accurately observed and correctly reported, and as to whether this or that fact may not be capable of a different interpretation from that which Dr. Bastian places on it. I am not concerned to enter on these matters, and I will say at once that I will make Dr. Bastian a present of all his facts. I will take his doctrine as he himself states it, and show, not merely that he has made out no case to be answered, but that he himself explicitly admits the accuracy and truth of the orthodox view.

Dr. Bastian says that these cortical centres are not the *real* motor centres. The real motor centres are in the cord and medulla. But what does he mean by a real motor centre? Those only, it appears, which co-ordinate muscular actions into movements. This alone is what Dr. Bastian calls co-ordination. But this is only one case of co-ordination. The combination of movements into acts is a case of co-ordination, and is effected by centres higher than those in the cord. The combination of acts into operations is a case of co-ordination, and is affected by centres higher than the last. The combination of operations into conduct is a further instance of co-ordination, and is effected by the highest centres of all. Every one of these co-ordinations is a co-ordination of movement, is a motor process, and is effected by motor centres. However, I do not desire to be exacting, and I will admit, for the sake of argument, that the only real motor centres are in the spinal cord.

Dr. Bastian's next point is that the cortical centres are not motor, but sensory. They are not motor, because, as we have just seen, the influence descending from them to the periphery has to act through other centres before producing movement. But is it pretended that sensory impressions on their way to the cortex are not co-ordinated and re-co-ordinated and co-ordinated again? and that in undergoing this process they have not to pass through centre after centre? If the motor centres are not motor because other centres intervene between them and the periphery, then for the same reason the sensory centres are not sensory. However, I will yield this point also, and will grant Dr. Bastian all his data, submit to all his arguments, and show that his conclusions, so far from being adverse to the generally-received doctrine, are in fact entirely in harmony with it and corroborative of it.

Dr. Bastian admits that the centres in dispute are the turning-point of the nerve current. He admits that although they are not motor centres, yet from them issue "volitional motor incitations." These volitional motor incitations descend to the "real" motor centres and set them in action, and hence arise movements. Here I have Dr. Bastian on the horns of a dilemma.

What is it that descends to the motor centres? A volitional motor incitation, (p. 87). A volitional impulse, (p. 54). What is that? Does Dr. Bastian mean that the will sits enthroned in the cortical centres and from thence directs the operations that go on in the brain? Does he mean that the will itself passes down the nerve fibres and discharges the cells of the lower centres? He has expressly repudiated any such absurdity. But if he escapes from this horn of the dilemma it is only to be impaled on the other.

If it be not a state of mind that descends to the cortical from the "real" motor centres, then it must be a physical process, a nervous discharge, or nerve current, or molecular movement of some kind. Now mark: this current is an efferent current; it is an efferent current which really is (p. 86) a motor incitation; that is to say, which results in a movement. And this efferent current is also a "volitional impulse;" that is to say, the nervous action is attended by the mental act of willing. Of willing what? A movement. In other words, this "volitional impulse," which on its physical side is an outgoing current, is on its mental side a volition of movement. Now it cannot be contended for a moment that an idea of movement does not form part of a volition of movement. I cannot will to walk across the room without having an idea of walking across the room. So that, according to Dr. Bastian's own statement, *the idea of movement is a concomitant of the outgoing current*, and this doctrine, which he set himself down to disprove, he has laboriously established beyond all risk of confutation. Like Balaam, the son of Beor, he took up his parable to curse, and he has blessed us altogether.

Dr. de Watteville.

From the discussion that precedes, we gather that those who refuse representation in consciousness to the purely motor, or outgoing, processes of the cortex, are not agreed as to the nature of the Rolandic area. Whilst Dr. Ferrier considers it as the cortical equivalent of the anterior horns of the spinal cord, Dr. Bastian argues for its sensory, or rather receptive, character, and attributes to it *kinæsthetic* functions, implying by this word that the so-called motor convolutions are the end stations of sub-conscious centripetal impulses from the organs of movement.

Between these two views that of Dr. Ross forms a transition. According to him, the large cells of the Rolandic area are truly motor, whilst the small cells are receptive, perhaps even sensory.

I do not propose to enter upon this aspect of the question now before us, for the simple reason, that I scarcely feel competent to discuss a point so full of difficulty. But I accept as more consonant with the facts the view held in common by the distinguished neurologists just named, and shared by many of the leading psychologists abroad, that the physical substrata of volition, of the feelings of effort and movement, are antecedent processes to the final, or "motor" cerebral discharge. Dr. Mercier has just given expression, in terms more lively than forcible, to a diametrically opposite opinion. He calls our position a "geocentric" one. I should have thought that, if anything deserved

this epithet in the matter, it was his own attitude with reference to the progress of our knowledge; for the fact of his having thrown in his lot with the "outgoing" school seems to have blinded him to the consideration that their number is but limited. Wundt's name is constantly bracketed with that of Bain in reference to the views he holds upon this subject; but I must confess that, on searching through the pages of the second edition of his 'Physiological Psychology' for clear evidence as to the German author's actual opinion, I failed to convince myself that full justice is done to him by this constant and close association of names. Be this how it may, however, I am afraid that what Dr. Mercier is pleased to call the "orthodox view," will, like many other orthodoxies in the present day, appear to every impartial mind as standing on its very last legs.

But if any arguments are required beyond those already adduced, to show that our conscious life is closely bound up to the activities of our sensory cerebral mechanisms, we might appeal to the fact that consciousness is less an end in itself, than a means towards further ends. It accompanies and appears to guide imperfect action; it tends to disappear with the development of more perfect mechanisms. The main function of sensory centres is to educate motor co-ordinations, the essence of which is their automaticity. Moreover, if discharges of the motor elements were really the substrata of volitions, of the feeling of effort, then we must assume a very deep separation between the mental representation of, and the wish to perform, a certain movement, on the one hand, and the fiat, or volitional act, on the other. Psychological analysis does not seem to me to admit of such a separation. Again, on the principle of differentiation of functions which pervades the evolution of organs throughout the animal series, we have a right to assume that the physical processes, of which consciousness is the subjective equivalent, tend to become more and more localised in circumscribed areas of the nervous system. Hence there is no *à priori* objection to the view which localises them in the receptive areas of the cortex. And in presence of an undoubted law of nature the speculative views of transcendental brain-physiology, such as the theory of the representation (whatever this term may mean) of every part of the body in every part of the cortex, must obviously recede into the background.

Indeed we are not so much concerned here with general arguments and *à priori* considerations as with the evidence of experimental facts. One of the main questions is whether destruction of the Rolandic or "motor" region of the human brain is or is not characterised by a diminution of muscular sense in the parts

paralysed; whilst another is whether such destruction is characterised by a concomitant loss of certain assumed psychical equivalents. Now, leaving for further investigation the former problem, it will be found that the facts at our disposal for the solution of the latter are but few; but, so far as they go, they certainly seem to support the view, that the activity of these centres is not largely if at all represented in consciousness. Lesions, at any rate unilateral lesions, strictly localised in the ascending convolutions, do not appear to be accompanied with more than feeling of mechanical inability to move the corresponding limbs.

The attempt to identify discharges of motor cells, or the "outgoing current," with the physical process of volition, involves a serious difficulty; for if it were so, every reflex process including such a discharge would thus, *ipso facto*, become a voluntary act. Nor do I understand why, if the subjective phenomena attending volition to move a limb after amputation (spectral hand) are mental equivalents of the motor cerebral discharge, similar phenomena do not accompany similar discharges in other cases, where the movement is similarly willed but not carried out—in spinal paralysis, for instance. This fact clearly points to the lesion of the sensory nerves, and consequent irritation of the sensory centres, as the probable cause of these hallucinations.

When I try to analyse the contents of my consciousness during the performance of a deliberately willed movement, I find myself in presence of a highly complex state, in which the senses of sight and touch enter for a very large share. I cannot really separate the visual representation of the moving limb from its numerous tactile accompaniments, such as contacts with clothes, air, surrounding objects, or other parts of the body. These elements, which form a large portion of what is called the "feeling of movement," depend obviously upon sensory activities of the brain.

But there is also involved in the state of consciousness described as the volition of a movement, the phenomenon of *effort*, which is itself reducible to simpler elements. It includes numerous afferent feelings; sensations of cutaneous and deep pressure; of strain on the muscles, tendons, and ligaments, not always confined to the limb directly involved, but often diffused over a larger area; there is also a sense of air pent up in the lungs so as to give a firm support to the thorax, of excited heart action, of throbbing arteries and venous congestion. Sooner or later the sense of spent energy, of fatigue, supervenes. All these elements may be accounted for by purely centripetal impressions. But in addition there is the distinct consciousness that I am engaged in the effort, that I am the cause of my movement. Now psycho-

REPLY OF DR. CHARLTON BASTIAN.

The task I have undertaken is, as I am fully conscious, one of extreme difficulty. I have endeavoured to show that a view widely accepted in this and in other countries, to the effect that there are motor centres in the cerebral cortex specially concerned with the execution of voluntary movements, is one which, if not actually erroneous, is, at least, not at all more probable than a counter view, which maintains that these so-called motor centres of the cortex are, in reality, sensory centres constituting the termini for "muscular sense" impressions. It has been through the experiments and writings of Dr. Ferrier, in the main, that the former view has gained so wide an acceptance in this country. Being strongly inclined to adopt the above-mentioned counter interpretation, as one capable of affording a much better explanation of known facts, it became of great importance for me to scrutinise very carefully all the steps by the aid of which Dr. Ferrier had arrived at his conclusions. It was, however, equally important, looking to the difficulty of my task, that full liberty should be accorded to me candidly to point out what I might consider to be defects, contradictions, or insufficiently warranted statements of any kind in the facts or arguments by which the opposite doctrine has been supported. Without this licence having been granted to me, no progress could have been made in dealing with so complicated a subject. Whilst deeply grateful for the consideration and attention which the Society has already accorded to me, I fear I must now crave no small measure of further indulgence.

It may be useful for me at this stage briefly to recapitulate some of the results of my scrutiny, and some of the points to which I have desired to call especial attention.

(1.) I was compelled to point out that my scrutiny of Dr. Ferrier's writings, bearing on this subject, led to the conviction that his doctrines did not hang together at all well; that they were even made up of contradictory statements.

(2.) I also came to the conclusion, that some of his fundamental positions, supposed to be based upon clinical evidence, were in reality faulty, being drawn from too narrow a view of altogether exceptional cases, or else resting upon inadequate observation and inquiry.

logically it might be maintained that I have reached cognizance of my own self as a cause through a process of inference; but this is not the place to enter upon such considerations. My object is to show how those who uphold the outgoing theory, and who object to the afferent doctrine on the ground of its supposed inability to account for this possibly primary datum of consciousness, may get over their difficulty and frame a hypothesis that does not postulate more than the activities of the receptive or sensory portions of the cortex as substrata of mental states generally.

Assuming that consciousness is the mental equivalent of the metabolic processes attending the activity of certain cortical elements, one might expect certain subjective states to accompany the molecular changes in the cell considered as a *terminus*, as it were, and as storing up impressions from the periphery; and other states to accompany the action of the same cell considered as a *switch*, or a *relay* for the further transmission of force. A physical basis might thus be found for dynamic as well as static conscious states, localised in the purely sensory regions of the cortex. And as will is, after all, but intellect in action—intellect itself being but the elaborated product of experience—it is not unnatural to look for the physical substratum of both cognitive and volitional phenomena in the same organs, viz. in the receptive and higher elaborative layers of the sensory cerebral sphere. Among the latter I include those tracts of cortical substance that preside over the complex inhibitions in which we must seek the physical basis of the psychological illusion described by introspective observers as the "liberty of the will," or the faculty of selection of the motives in human actions.

Whether this theory is sufficient to account for all the aspects of the dynamic states of consciousness (including the feeling of fatigue alluded to by one of the previous speakers), is not material to the present argument. It is at least as plausible as that which postulates the activity of purely motor-cortical elements as the embodiment of our mental states in voluntary movements. It is suggested here merely as an alternative hypothesis for the consideration of the supporters of the "outgoing" theory of the muscular sense. Neither they nor their opponents have as yet, so far as I know, considered discharges of *sensory* elements in the light in which I have endeavoured to place them. Should the attempt lead to nothing but a refutation, I shall yet congratulate myself on the opportunity afforded to elicit further discussion of a subject as important as it is interesting.

he came to chap. xi. he expressed himself in the most emphatic manner as if he had still been an adherent of that doctrine of Wundt and Bain which he had already so successfully combated.

In the second edition of the 'Functions of the Brain,' without comment or explanation, the expressions last referred to were radically altered. Yet Dr. Ferrier seems to be unaware of it. He now says, "I have made no change of front, and am glad that in the second edition of my work I have succeeded in clearing away any misconception that may have existed as to my real meaning, which is, that I consider that the activity of motor centres and motor nerves by themselves is entirely outside the sphere of consciousness." Now let us see how far Dr. Ferrier can really claim to have been the pioneer in 1876 of that doctrine which I clearly enunciated in 1880—by placing side by side an original passage, and the same passage altered as it now stands in his second edition.

'Functions of the Brain,' 1st Edition, p. 266.

"In the same manner as the sensory centres form the organic basis of the memory of sensory impressions, and the seat of their representation or revival in idea, so the motor centres of the hemispheres, besides being the centres of differentiated movements, are also the organic basis of the memory of the corresponding movements, and the seat of their re-execution or ideal reproduction. We have thus a sensory memory and a motor memory, sensory ideas and motor ideas; sensory ideas being revived sensations, motor ideas being revived or ideal movements. Ideal movements form no less an important element in our mental processes than ideally revived sensations."

'Functions of the Brain,' 2nd Edition, p. 436.

"In the same manner as the sensory centres are the organs of special sensory perception, and the organic basis of the memory of sensory impressions, so the motor centres of the cortex, besides being the organs of differentiated movements, are the organic basis of motor acquisitions. But while the activity of the sensory centres reveals itself subjectively in consciousness as ideation or feeling, the activity of the motor centres has no subjective side apart from the functioning of the sensory centres, with which they are associated. Modifications of consciousness are correlated exclusively with the functioning of the sensory centres of the cortex."

It seems to me difficult to imagine any more complete change of front than is represented by these two passages, and if Dr. Ferrier does not admit it, I am afraid all will consider him hard to be convinced.

But let us turn to another point. In support of his fundamental statement, that "co-ordination is possible in the entire absence of any sense of movement," Dr. Ferrier can still only refer to one very exceptional case, that of Remigius Lens, which does seem, as I have already admitted (p. 15), whatever the explanation

(3.) I had to contend that experiments upon animals were quite useless for throwing light upon the question of the situation of the centres for muscular sense, and that all previous conclusions thus arrived at were valueless; that we must, instead, look minutely into the effects of cortical disease, and, better still, into the effects of excision of portions of the cortex in man, in order to ascertain whether or not lesions in the so-called motor area are attended with any corresponding loss of muscular sense.

(4.) Further, I stated my belief that Dr. Ferrier seemed not to have comprehended the all-important difference between the symptoms that would result from a mere cutting across of the afferent tracks for muscular sense impressions, and those that would result from destruction of the cortical termini for such impressions.

(5.) I endeavoured to show that Dr. Ferrier had taken an inadequate and indeed erroneous conception of the functional importance of kinæsthetic centres, and that upon them, in reality, would devolve just such functions as those which he supposes to be carried out in cortical motor centres.

(6.) I maintained that neither on physiological nor on psychological grounds was it needful to postulate the existence of motor centres in the cortex.

(7.) I also argued that the doctrine of the existence of motor centres in the cortex would probably never have been heard of had it not been for the importance previously attached to a doctrine now disproved, viz. that of Wundt and Bain, to the effect that our feelings of effort are due to psychical processes directly correlated with the activity of motor centres—a doctrine which Dr. Ferrier absolutely rejects, though it is still held, and has always been maintained by Dr. Hughlings-Jackson, the original propounder of the notion that motor centres exist in the cerebral cortex.

All that Dr. Ferrier has said in reply will be found to have reference to some of the points which I have referred to under the first three headings above mentioned.

In support of my position, that the doctrines upon which Dr. Ferrier originally based his conclusion that motor centres existed in the cortex were mutually contradictory, I called attention, as far back as 1880,[1] to the fact that whilst in chap. ix. of his 'Functions of the Brain' he thoroughly adopted and did much to establish the view, that our sense of movement was altogether dependent upon centripetal impressions, strangely enough when

[1] 'The Brain as an Organ of Mind,' 1st ed. p. 539.

may be, to lend support to his statement. The rule, however, is, as I contend, that the loss of muscular sense distinctly impairs the power of accurately co-ordinating movements when the eyes are closed. I, of course, do not mean to say that inco-ordination of movement may not be brought about also in other ways, and from what Dr. Ferrier says it would seem to be needful to remind him of this. Again, I maintain that frequently in cases of hemianæsthesia there is no loss of muscular sense, and no inco-ordination of movement; but that in a much smaller proportion of cases there is, in addition to the cutaneous anæsthesia, a profound loss of muscular sense (pp. 11–22). Dr. Ferrier says he is "amazed" at these statements, and adds, "I do not know a single observer, with the exception of Dr. Bastian, who has ever seen a case of hemianæsthesia in which muscular sense was not abolished or impaired in proportion to the degree of the anæsthesia existent." After what Dr. Ferrier has heard from Dr. Ross this evening, he will, at all events, have learned that the experience of that distinguished observer is in accordance with my own. If time and space permitted I could easily quote the observations of several other distinguished physicians to the same effect. I trust, however, that two or three such quotations may suffice to convince Dr. Ferrier that it is his own view and not mine which is most calculated to excite surprise in the minds of others.

Dr. Russell Reynolds has made the following remarks[1] concerning cases of marked hemianæsthesia under the care of M. Charcot, which he had had the opportunity of examining for himself:—"There was no deficiency of knowledge as to the position of the limbs; but M. Charcot informed me that he had met with this deficiency in other cases." Again, Dr. Arthur Gamgee writing[2] concerning hystero-epileptic patients whom he had examined in M. Charcot's wards, speaks in the following terms of this affection:—"It is associated with hyperæsthesia in one or both ovarian regions, and is usually attended by hemianæsthesia, and more rarely by anæsthesia of both sides of the body. There is some, if not complete, loss of tactile sensibility, and usually absolute insensibility to pain (analgesia) of skin and all other sensitive structures on the affected side; the muscular sense being, however, nearly always preserved." Let it not be supposed, however, that the recognition of differences in the degree of hemianæsthesia of the kind to which I have referred, is a result only of modern observation. I would call Dr. Ferrier's attention to the fact, that the view opposed to his own was clearly substantiated by

[1] 'Lancet,' May 12, 1877, p. 679.

[2] 'Brit. Med. Jour.,' Oct. 12, 1878, p. 545.

Duchenne more than thirty years ago. In his work 'De l'électrisation localisée' (p. 410) this accurate observer calls attention to the existence of two categories of hemianæsthesia; one (*a*) in which there is loss only of cutaneous sensibility, and another (*b*) in which there is, in addition, loss of the sensibility of the muscles, bones, and nerves. In regard to representatives of the former category, he says:—"Ils ont la conscience des mouvements mécaniques qu'on leur imprime, de l'étendue des mouvements qu'ils exécutent eux-mêmes, de la pesanteur, de la résistance; chez eux, enfin, la contractilité volontaire n'éprouve aucun trouble dans son action physiologique." In his description, however, of a case belonging to the second category he says:—"Quand il est dans l'obscurité, ou si on l'empêche de voir, il n'a pas la conscience de la position de ce membre, ni des mouvements les plus brusques qu'on leur imprime." He then goes on to point out that this second category includes two sub-varieties, in one of which (1) the patients are quite able to execute movements when their eyes are closed, though they have no consciousness of the extent of such movements and are unable to estimate weight or resistance; whilst in the second sub-variety (2) the patients (under similar conditions), though presenting the defects above-mentioned, lose also their power of executing even the simplest voluntary movement. Of the latter class I have quoted three very striking examples, recorded respectively by Duchenne, Briquet, and Bazire (pp. 16–20).[1]

[1] Since Duchenne's time no one has attempted to give an explanation of these remarkable cases excepting that which I have suggested (p. 36). I have supposed that in them we have to do with a functional defect (that is a state of lowered irritability) in the cortical termini for "muscular sense" impressions, associated with some defect in the functional integrity of the afferent channels for such impressions, as they pass through the posterior part of the internal capsule. According to Duchenne, it was the absence of what he termed "la conscience musculaire" which determined the inability to perform the simplest voluntary movements in this class of cases (*loc. cit.* p. 414). Now it seems clear that the revived activity of kinæsthetic centres, such as occurs in the conception of a movement, would exactly correspond in the time of its operation with what Duchenne termed "la conscience musculaire," which he says "dans l'acte des mouvements volontaires, semble précéder et déterminer la contraction." Duchenne found that the missing power in his cases might be restored by very strong faradisation of the muscles (which sufficed to restore sensibility to them) even though the cutaneous anæsthesia persisted. At the same time that sensibility was thus restored to the muscles it is easy to imagine that the powerful peripheral stimulation (faradisation), acting upon the sensory nerves of muscles, might easily have sufficed to rouse their previously dormant cortical termini into activity, and thus may we account for the restored ability to perform movements when the eyes are closed. Thus, also, may be explained the frequent similarly good effects of powerful faradisation of the affected muscles in some cases of hysterical

Another firmly rooted belief held by Dr. Ferrier is the converse of that last referred to, viz. that cases are never met with in which muscular sense is lost apart from loss of tactile sensibility, or altogether out of proportion to the loss of tactile sensibility. I fully admit that few definite records of such conditions exist, but I am far from believing that they will not be found when they are carefully looked for. Thus, I think it may be possible to induce such a condition in part artificially, by dealing with a case of functional hemianæsthesia of the second category, after the fashion adopted by Duchenne, as referred to in the note on the previous page—that is, by strongly faradising the skin so as to restore sensibility to it whilst the muscles are this time left in their anæsthetic condition. Though Duchenne says he has several times done this, he has left no specific statements on record as to the continued existence of loss of knowledge of the position of the limbs, and of their passive movements, in such cases. He does, however, state that in the instances of the second sub-variety in which he has performed this experiment, the patient's inability to move the limbs voluntarily without the aid of sight impressions has persisted when sensibility has been restored to the skin, so that it becomes highly probable, to me at least, that the loss of so-called muscular sense would also have persisted under these same conditions.

In illustration of this kind of defect I have, however, in my paper suggested that we should search for it under a different set of conditions—that is, in cases of a wholly different type. The conducting tracks for cutaneous and for muscular impressions seem to be more or less separate from one another, though contiguous in their passage through the internal capsule, but I suggest that they proceed, in the main, to entirely separate cortical stations. My provisional hypothesis, in fact, is that the convolutions comprising the so-called "motor area" are in reality the cortical termini for muscular sense impressions. On this point, as I fully admit, evidence in actual proof has yet to be obtained, though I have attempted to show how well this hypothesis is capable of explaining known facts. As strongly bearing upon this problem, I contend that observations on the lower animals are worthless for the settlement of the location of this endowment. We must look to cases of cortical disease in man, and especially to cases in which, owing to disease, the so-called arm and leg centres (but especially the former, on account of the

paralysis—a condition which may be supposed to depend, as I have suggested (p. 36), upon still lower conditions of functional irritability in corresponding muscular sense centres.

greater facility with which defects of muscular sense may be detected in the arm and hand) have been excised by the surgeon.

With this object I quoted a few details of two cases briefly reported by that excellent observer Landry (who had made defects of sensibility of different kinds a special study), in which, he says, there was marked defect of muscular sense, and but little, if any, defect of cutaneous sensibility, as possible cases (looking to all that is said concerning them) of cortical disease in the Rolandic area. Similarly, as still more suggestive, I cited a case recorded by P. Zenner (which was kindly brought to my notice by Dr. de Watteville); and, as amounting to something more like proof, so far as they went, I also cited two cases of excision of portions of the cortex which had been recorded by Mr. Horsley. One of these latter patients was in part under the care of Dr. Ferrier himself, and, as we have heard, he thinks nothing of the evidence either of them may afford in support of my hypothesis. But after weighing all the doubtful elements which they presented, Mr. Horsley is not of that opinion. He has expressed, indeed, to-night the same kind of conclusion that I have drawn when he says:—"I would not rely on their evidence further than to regard them as tending to show that the so-called motor region is the centre of muscular sense."

Dr. Ferrier, however, refers to another case of operation for disease in the right Rolandic area by Mr. Horsley, which seems to tell against the above-mentioned conclusion. I had not the opportunity of seeing the patient in question myself, who is referred to by Dr. Ferrier as constituting "a crucial experiment, which absolutely knocks the ground from under Dr. Bastian's feet." It may well be imagined, however, that this case was examined and considered most carefully in all its aspects by Mr. Horsley, and the terms in which he has spoken this evening concerning the man, referred to in such positive language by Dr. Ferrier, are altogether different. He says:—"The whole surroundings of the case are so very complicated that even after the death of the patient I should not like to adduce it alone in support of an argument one way or the other."

I adduce the cases above referred to only for what they are worth. We must undoubtedly wait and seek further evidence. I feel this necessity fully, though I hope to have made out a sufficiently strong case to lend an increased stimulus to the minute observation of cases of disease in, or removal of, parts of the excitable area of the cortex. Certain it is that Dr. Ferrier's views do not lend any encouragement whatsoever to the notion that there would be loss or diminution of

muscular sense from disease in, or excision of, these regions of the cortex. While, on the other hand, if my hypothesis be true, it follows, as a necessary consequence, that in such cases there should be evidence of some loss or defect of muscular sense (see p. 73) in association with motor paralysis. There might or might not be, in addition, a certain amount of loss of tactile sensibility, such as was present in the case of Zenner. Such a combination might be met with if all tactile impressions do not proceed, as Dr. Ferrier supposes, to the falciform lobe as their cortical terminus. That is, in my opinion, also a point which requires to be carefully looked to in the future, especially in view of the statements made by Munk.

Dr. Ferrier reiterates his belief, that the cortical centres in the excitable area are true motor centres; but he does not adduce a single reason tending to displace my contention, that all known facts can, as I have endeavoured to show (pp. 80–84), be equally well explained in accordance with my hypothesis. Nor does he attempt to controvert any other of the positions which I have supported.

I am happy to find that between Dr. Ross and myself there are many points of agreement. Thus, he is at one with Dr. Ferrier and myself in regard to the fundamental point, that the sense of effort or of resistance is correlated with ingoing currents and the activity of sensory centres; he agrees with me, in opposition to Dr. Ferrier, that in the majority of cases of hemianæsthesia there is no correlative loss of the "muscular sense" endowment; he is further in agreement with me (and with Dr. Ferrier's later view) in regard to the absence of psychical states in association with the activity of motor centres. There is additional agreement between us in regard to another point, on which Dr. Ross says, "I at least give my unhesitating adhesion to Dr. Bastian's opinion when he says that the kinæsthetic centre is the last centre which must be excited anterior to the movement in any psychical action." There is even a readiness on the part of Dr. Ross to find that the kinæsthetic centres are situated where I believe the most important part of them to be located, viz. in the so-called excitable area of the cortex, since he says:—"I think it, however, quite likely that the kinæsthetic and motor centres coincide, in so far as that the former are situated in the two outer and the latter in the third layer of the cortical cells of the parieto-frontal area of the cortex."

One most fundamental difference, however, separates the views of Dr. Ross from my own. I read with genuine surprise the statement in which he says:—"I do not believe that any amount of repetition tends to convert a movement which was once

voluntary into a reflex action either in the individual or the race." Yet a further study of all that he says concerning movements has shown me that this statement (apparently so opposed to the philosophy of Evolution in which Dr. Ross is a firm believer) is due to what I take the liberty of terming arbitrary limitations that he has chosen to give to the words "voluntary" and "reflex" respectively. I am by no means convinced that I have been so very wrong in making use of commonly accepted terms, or that Dr. Ross is so right as he imagines in his use of his own terms and in his classification of movements. This, however, is a side path, into which I will not be seduced, even in order to defend myself from the strictures of Dr. Ross. Elsewhere I have pointed out more in detail what meanings I attach to the different terms commonly used in the classification of movements,[1] and I am not aware that the least ambiguity has crept into my argument on the present occasion from the use which I have made of these terms.

A view has been broached by various persons of late to the effect, that the excitable area contains both sensory and motor centres, rather than motor centres only, as Ferrier contends, or sensory centres only, as I am more inclined to maintain. This view has to-night been mentioned favourably both by Dr. Ross and Mr. Horsley. They are not indisposed to believe that the upper small-cell strata of the Rolandic convolutions may be concerned with sensory functions of the kinæsthetic type, though both of them seem strongly wedded to the notion, that the large cells in the deep layer are real motor cells—in the main because of their complete organisation, large size, and analogies with motor cells in the anterior cornua of the spinal cord. I must confess I do not sympathise with this notion, though I do not by any means regard it as an essential part of my hypothesis, that the whole depth of grey matter in the excitable area should be given over to kinæsthetic functions. Some evidence already in our possession tends to show that tactile sensibility may also be, to some extent, represented in this area; and should this evidence be strengthened, I shall be quite prepared to believe that the upper layers of the cortex of the Rolandic area are concerned with the reception and revival of these impressions, while the lower layers are concerned with the reception and revival of kinæsthetic impressions. I am not influenced by the mere fact of the large size and elaborate structure of the cells of the deep layer, as Dr. Ross and Mr. Horsley seem to be, and that for two sets of reasons. First, I fail to see any necessity for the existence of motor cells in the cerebral cortex, because I believe that volitional action, so far as it is dependent

[1] 'Brain as an Organ of Mind,' p. 459.

upon cortical substrata, is essentially intellectual action, in which the activities of motor cells take no direct share. On this part of the subject I shall have something to say further on. Neither Dr. Ross nor Mr. Horsley adduces anything, from this point of view, in support of a necessity (which possibly they may feel) for postulating the existence of motor centres in the cerebral cortex; they dwell rather upon the histological analogies existing between the large cells of the cortex and those of the motor regions of the spinal cord. But this second set of reasons has no cogency for me. I mistrust such analogies altogether, knowing how deceitful they have often proved. Then, again, I think it not improbable that the large size and marked development of these cells may be as much a necessity for them (in view of the kind of functions I suppose them to subserve), as for the motor cells in the anterior cornua of the cord. It should, further, be remembered by Dr. Ross that, in accordance with my view, the psychical states associated with the activity of these cells are non-vivid, vague and comparatively faint, and that if there are (as I suppose) no motor cells in the cortex, the large cells in question there situated would, as constituents of kinæsthetic centres, have functions to perform of a kind wholly different from those of the constituents of all other cortical sensory centres; since it would be from them alone that efferent incitations would pass over from the cortex for the excitation of motor centres of whatsoever grade. Thus it would happen that in many cases molecular movements would have to be transmitted from different groups of these kinæsthetic cells, now to the cervical, now to the dorsal, and now to the lumbar regions of the spinal cord, through sets of internuncial fibres correspondingly increasing in length. This kind of function may well necessitate a cell of large size; so that I cannot feel that the argument from mere histological characters carries any weight with it, or can be said, when looked at more closely, to tell more strongly in favour of the one than of the other hypothesis.

Dr. Ross says:—"The main question which divides Dr. Bastian from Dr. Ferrier is the point at which the centripetal conducting paths and centres cease and the centrifugal paths and centres begin. Most of us will at once say that this point is to be found in the large caudate, pyramidal cells of the third layer of the cortex." That is not, to my mind, an adequate statement of our difference; else it would not be possible for me to say, as I do, in reply to Dr. Ross,—I also believe these cells to be the point where centripetal paths and centres cease, and where centrifugal paths begin—if it be permissible to consider as centrifugal the path which connects sensory with motor elements in an ordinary sensori-motor unit.

Mr. Sully, Sir James Crichton-Browne, the President, and Dr. Mercier all more or less strongly support the notion of Bain and Wundt, that psychical processes are directly associated with the activity of motor centres and nerves, and that it is in and through them, in the main, that we derive our notions of weight and resistance.

Now it was because I believed this view to have been absolutely disproved, that I devoted comparatively little attention to it in my paper. It seems to me, as I pointed out long ago, that the cases of hemianæsthesia of Duchenne's second category (pp. 11–20) suffice of themselves completely to displace this doctrine. Referring to the case of Demeaux, I then said[1]:—"There was in this woman a total disappearance of all that kind of knowledge which has, by one or other, been ascribed to, or supposed to be derived from, the 'muscular sense.' The woman was ignorant of the position of her limbs and unconscious of any movements that she might execute. The volitional centres, the spinal motor centres, the motor nerves, and the muscles were capable of being called into activity as before—yet all the information supposed to be derived through the 'muscular sense' had vanished." . . . . "No clearer evidence than this, together with what has been previously mentioned, could be forthcoming to show that the knowledge of the position of our limbs, of their movements, and of the state and degrees of contraction of our muscles generally, does not depend, as Wundt, Bain, and others assume, upon impressions that are 'concomitants of,' or that coincide with, 'the outgoing stream of nervous energy.'"

But if anything further be needed to convince those who still have a lingering affection for this doctrine above referred to, other strong facts in condemnation will be found in Dr. Ferrier's 'Functions of the Brain,' and in Dr. James's memoir on 'The Sense of Effort.'

All that Mr. Sully says concerning the hallucinations that are often known to follow the loss of a limb, can be fully explained by reference to the irritation of sensory nerves and sensory centres (pp. 37–40). That this is so derives strong confirmation, as Dr. de Watteville points out, from the fact of the total absence of any such hallucinations in cases of complete anterior poliomyelitis, where what is practically a maiming occurs only on the motor and not at all on the sensory side of affected limbs.

Nor can I think that any real light will be thrown upon the fundamental problem by mere speculations concerning the very doubtful pathogenesis of "fatigue and fidgets." I will not attempt, therefore, at any length to follow Sir James Crichton-Browne in

[1] 'Brain as an Organ of Mind,' 1880, p. 700.

his speculations upon these subjects. Still it may be remarked that the "fact of the indisposition for mental as well as for bodily exertion" under conditions of fatigue would of itself seem to point to some general modification in the great nerve centres, and not to an altered molecular condition only of hypothetical "motor centres" in the cortex. Similarly indecisive, as it seems to me, is his supposition in regard to the causation of fidgets, which he imagines to be due to a "surcharged or excitable state of the same centres." It is indecisive, I mean, for helping us to gauge the relative correctness of his view and of mine in regard to the true functions of these particular cortical centres, seeing that it would be equally open to me (holding the views which I have brought forward as to the functions of these centres) to explain with the same degree of plausibility the phenomena of fidgets by the phrase above quoted, adduced though it was by Sir James Crichton-Browne in support of the rival doctrine.

I believe, however, that such speculations are too vague to be of any service to either of us. When Sir James Crichton-Browne says, "I cannot divest myself of the belief that as we have perception at the end of ingoing currents, we must have apprehension at the beginning of outgoing currents," I see in that statement the enunciation of a favourite belief rather than evidence in support of its truth.

Again, I am afraid, Sir James Crichton-Browne has read my paper to little purpose if he supposes that I am willing to assent to his notion, that to endow the convolutions of the "excitable area" with kinæsthetic rather than with motor functions is "to degrade a large region of the cerebrum from its high estate, and leave it a mere superfluous intrusion in the brain mass;" or if he imagines that it forms any part of my view to say that kinæsthetic centres have "no part to play in ideation," or that they are, in any way, "redundant or superfluous." In regard to the performance of voluntary movements, Sir James Crichton-Browne says, "Sensory impressions from the part to be moved can play no part. They come too late." That is true. But what I contend is, that it is the molecular motions concerned with the memory of past visual and kinæsthetic sensations occasioned by similar movement, whose revival takes part in the production of the movement now about to be executed. These centres are, in fact, those concerned with what he terms "the regulation of the jet," and that, through the intervention, in part, of revived impressions, and, in part, of now occurring impressions occasioned by present movements.

Dr. Hughlings-Jackson, in the main, reiterates his well-known views, but unfortunately, in spite of them, or rather after

having given them the fullest consideration, I have felt compelled to come to the very opposite conclusions. He does not in any way endeavour to show me where I am wrong, or attempt to explain away the difficulties which, as I have pointed out, now make his favourite doctrine untenable. I am as fully imbued, perhaps, as he is with the great value of the doctrine of Evolution, and have been so for the last five-and-twenty years at least. I agree with him further, that "the unit of constitution of the nervous system is a sensori-motor unit." But I can neither join with him in supposing that we have any knowledge whatsoever of psychical accompaniments of the molecular processes occurring in motor centres; nor that there is the least need, in accordance with the doctrines of Evolution, for supposing the existence of motor centres in the cerebral cortex. Much of that knowledge which he ascribes to molecular movements taking place in motor centres, I believe may be better accounted for by molecular movements taking place in sensory centres. The activity of sensory centres constitutes in all cases, as I maintain, the necessary prelude to, and the guidance by which, this or that kind of activity is to be awakened in the constituent elements of motor centres, for the production of purposive movements. Dr. Hughlings-Jackson says:—"I contend that when I make a movement with my hand I have a psychical state just as certainly as I have another when I see that red chair." In this I quite agree with him, only I say that the psychical state is compound in its origin, and of the kinæsthetic order. But when he says, "I submit that we have psychical states attending activities of motor elements," I do not at all agree with him, seeing that he has left unanswered all the overwhelming objections to this doctrine—and especially if the assertion which immediately follows is to be taken as a type of the only evidence he can adduce in its support. What he subsequently adds is, in my opinion, of equally doubtful value. He says:—"For these psychical states there is no name except in the case of words; words are psychical states attending activities of motor elements representing certain very special and complex articulatory movements." But to make such a statement without proof as a contribution to this discussion, when Dr. Jackson knows that it is a view directly repudiated by myself and many others as an appanage of the above-mentioned disproved doctrine, is no more likely to weaken the tenability of my view than to strengthen his own. Merely to reiterate such statements is surely useless when the underlying doctrine is itself fast dying if not actually dead.

Again, how what Dr. Jackson says touching the errors of the

"popular psychologist" can be supposed to throw light on this discussion I fail to understand, as I do not know whether or not he imputes to any one who has taken part in it the crass absurdities which he mentions.

I find it a little difficult fully to appreciate the intended cleverness of Dr. Mercier's contribution to this discussion, because it at once impresses me with a sense of its hollowness. Like a bubble, it only requires to be pricked to vanish into thin air.

Dr. Mercier is supremely indifferent as to facts for the establishment of his own views, and correspondingly inaccurate in his interpretation of my views. He professes to define my notion as to what constitutes a real motor centre, and also to state what I mean by co-ordination, though in neither case is the result such as I can accept as a statement of my view. He seems to think his own notion that all co-ordination of movements occurs in motor centres of higher and higher rank, from the cord upwards to the cerebral cortex, must necessarily be true. Here, however, I venture to differ from him in two respects. First of all, I do not accept as motor his motor centres of the cortex; and, secondly, I say that co-ordination of movements mainly takes place in, or is brought about by, different sensory centres, of higher and higher rank and endowed with more and more complexity of function, the lowest being in the spinal cord and the highest in the cerebral cortex. Further, I am of opinion that it is under the influence of these sensory centres of different grades that motor elements in different motor centres are called into simultaneous or successive activity, as a result of which connections become established between them (motor co-ordinations), with the effect that subsequent movements of the same kind are rendered more and more easy. Co-ordination is, therefore, in my opinion, primarily and essentially dependent upon the functional activity of sensory centres, though the motor centres respond secondarily by the establishment of intercellular and intercentric connections.

Again, the reason which Dr. Mercier imputes to me as the support for my belief, that the centres of the "excitable area" are not motor, is not in the least in accordance with what I say. I have sought, on the one hand, to bring forward some evidence tending to show that these centres are the termini for a certain class of sensory impressions, and on the other, that there is no need for postulating the existence of cortical motor centres at all.

After such various statements of his own and not too accurate representations of my views, Dr. Mercier approaches the climax of his argument not without signs of exultation. He triumphantly proceeds to show how, in his opinion, I have performed the

remarkable feat of establishing in an incontestable manner the doctrine which I sought to refute. Here again he must permit me to differ from him. I am seemingly led to this inane result by dint of a little jugglery on the part of Dr. Mercier with my words, coupled with a *suppressio veri* which is equally disingenuous. Thus, by attributing to me the view, that the efferent current from the "excitable area" is attended by an act of willing, he seeks to make me say that "what on its physical side is an outgoing current, is on its mental side a volition of movement," whereas I never said anything of the kind. My contention is that the act of willing immediately precedes the outgoing current, and that the molecular movements which constitute this current are in part engendered in, and receive their final direction from, the functional activity of *sensory* centres of kinæsthetic type entering into the composition of the excitable area of the cortex.

Now what Dr. Mercier erroneously terms the commonly received doctrine (i.e. the disproved doctrine of Bain and Wundt) is, that some of the sensations accompanying movements are concomitants of the outgoing current from *motor* centres through whose activity they are realised in consciousness; whereas my contention is that all the sensations accompanying and occasioned by movements are realised, through the intermediation of sensory nerves, in sensory centres. Yet Dr. Mercier is pleased to say that I have proved the very doctrine I set myself to controvert. I fancy few will be found to agree with him.

The remarks of Professor Haycraft are interesting, and it is not difficult to recognise in them the expressions of a genuine seeker after light. When he says that a sensory impulse from the arm "passes to the brain in order that it may be correlated with other impulses passing in other loops belonging to the special senses; for the brain is to be considered strictly as an enlargement due to the presence of those sense organs," I am quite prepared to agree with him. Much of what follows, however, seems to me to be either obscure or of doubtful cogency. I do not think we can accept his definitions of a sensory cell and of a motor cell, since they would apply, for the most part, only to certain cells in the spinal cord or medulla. But whenever an impression of a sensory order ascends through the spinal cord to the brain, it may and probably does pass through two or three sensory centres, in which it may be brought into possible correlation with other afferent impressions, before reaching certain groups of cells in the cortex. Now these latter would seem to be typical sensory cells, since their activity immediately lights up a correlated activity in many

parts of the brain, the whole being attended by the development of certain psychical states corresponding with sensory and perceptive processes. But the activity thus excited may extend in still wider and more complicated circles throughout the brain, into regions which constitute (if we may so speak without implying anything like topographical exclusiveness) annexes of the sensory centres. The combined simultaneous activity of large portions of the whole of these brain areas would correspond with definite intellectual exercises such as the formation of general notions and concepts, together with abstract reasoning generally. All these portions of the brain must, however, be in the closest functional relation with those portions of the auditory, visual, and kin-æsthetic centres in which corresponding impressions connected with words in their auditory, visual, and kinæsthetic relations have been organised. By this kind of association language and the power of thinking are mutually strengthened, and undergo a co-ordinate development.

Thus, here in the cerebral cortex, as a whole, we have the birth-place and home of man's intellectual, moral, and emotional nature—the physical substrata being cells of sensory type and others more or less modified, together with the inter-communicating networks of all kinds by which those cells are brought into relation with one another.

But, it may be said, is there not a part of the cortex devoted to volitional action, and does not this part contain motor centres and motor cells? This is a view which is popular at present, and it is the position which Dr. Ferrier, following the lead of our President, has done his best to establish. For my own part, I regard every portion of this doctrine as erroneous. I am glad to find in Dr. de Watteville (with whose remarks I, for the most part, cordially agree) one at least ready to hold with me that volition is only intellect in action. This doctrine I have long held; therein following Spinoza, who said more than two centuries ago: —"The Will and the Intelligence are one and the same thing." From this point of view, as I have elsewhere contended,[1] Dr. Ferrier seems to start with a fundamental misconception when he supposes, in reference to cortical centres, that those "immediately concerned in effecting volitional movements" are "as such truly motor."

The scope of "Will" or "Volition" was clearly enough marked out by Locke when he said, in the simple but effective language of the time:—"We find in ourselves a Power to begin or forbear, continue or end several Actions of our Minds and Motions of our

[1] 'Brain as an Organ of Mind,' p. 581.

Bodies, barely by a thought or preference of the Mind." As to the nature of the processes comprised under what is known as "Will," may we not find them described in the simplest terms by Hartley and his predecessor Hobbes? Thus, Hartley says:—"The Will appears to be nothing but a desire or aversion sufficiently strong to produce an action that is not automatic, primarily or secondarily. . . . The Will is, therefore, that desire or aversion which is strongest for the present time." The conflict of mental moods or motives is sometimes slight and sometimes complex (entailing what we now term inhibitory processes), before what is called a resolution or Will to do a certain action is arrived at. As Hobbes quaintly says:—"The whole sum of desires, aversions, hopes and fears, continued till the thing be either done or thought impossible, is what we call Deliberation." Here then we have intellect in action, with absolutely nothing of motor activity concerned with its manifestation.

The result of any conflict of motives, great or small, simple or complex, is that ultimately our desire to carry into action one of a certain number of plans, or to operate in this or that way, triumphs. Supposing, for the sake of argument, that the result of our deliberation is a resolve or desire to perform a certain definite muscular action; we could not resolve to perform such action without having remembered the action in question, or, in other words, without having revived in idea a conception of the movement to be executed. This was long ago clearly pointed out by James Mill, who showed that something else in reference to volitions (or actions desired to be accomplished) always accompanies or immediately follows the emotion of Desire—viz., an Idea or Conception of the kind of Movement needed for the gratification of the Desire.

Now this idea or conception of the movement needed involves the conjoint activity of two sensory centres, that is of the visual and of the kinæsthetic (under the latter would be included those tactual elements referred to by Dr. de Watteville). This idea of the movement about to be produced seems to constitute, as James Mill contended, "the last part of the mental operation," that is the last actions associated with psychic processes—beyond we have mere physical links intervening in internuncial fibres, motor centres and motor nerves, through the intermediation of which appropriate muscular contractions are caused.

Thus, the exercise of our Will means the exercise of our Intellect till, in the end, we determine upon some course of action. There is thus no separate region of the cortex needed to be set apart for volitional exercise (the idea is absurd except to those who believe

in the doctrine of Wundt and Bain) simply because all departments of mind are liable to be called into action at different times in different combinations, as the prelude to our willing this or that action—and, consequently, wide but ever-varying portions of the cerebral cortex are called into play on different occasions. Such philosophers as Dugald Stewart and Dr. Thomas Brown deliberately omitted to discuss "Will" as a distinct department of mind. "To know all our sensitive states or affections," the latter says, "all our intellectual states, all our emotions, is to know all the states or phenomena of the mind."

Suppose, for the sake of argument, that my position be correct, that the whole of the cerebral cortex is occupied with such processes as those to which I have above referred. Then it must be clear that, from this cortical mantle, fibres must descend to motor centres of various grades of complexity situated in lower portions of the brain and in the spinal cord. That being so, the question at once arises, whether these efferent fibres would pass off from all sensory centres indiscriminately. That notion I reject. I come to the conclusion, that the most important sensory guidance and education of movements has its birth either in combined auditory and kinæsthetic centres (for articulatory movements), or in combined visual and kinæsthetic centres (for limb movements).

In the case of the activity of each of these couples I come to the further conclusion, that the kinæsthetic centres are the last to come into operation. And here I am glad to have the support of Dr. Ross, who says:—"I at least give my unhesitating adhesion to Dr. Bastian's opinion, when he says that the kinæsthetic centre is the last centre which must be excited anterior to the movement in any psychical action." But the very heart or kernel of the kinæsthetic centre in any one hemisphere would, in my opinion, be represented by the cortical terminus for "muscular sense" impressions. From such regions, therefore, it is, as I suppose, that the outgoing fibres issue by means of which our Intellect plays upon subjacent motor centres when we desire to perform this or that so-called "voluntary action."

Now for one word concerning the "fiat" which Sir James Crichton-Browne seems to surround with some air of mystery, as though there at last we had a something of cortical origin which could truly be described at once as psychical and as motor. This, in my opinion, is altogether erroneous. When we hesitate as to what we shall do or say and at last decide to adopt this or that alternative, there must already during the period of hesitation be nascent excitations in the parts of the sensory centres related to the

movements thought of (that is, rival nascent ideas of movement), which would correspond with different outgoing routes for the passage of stimuli to motor centres. But as soon as we arrive at a definite desire to perform one of the movements in question, all inhibitory influences blocking that route cease (we are at the stage of what has been termed the "fiat"), the idea of this movement becomes fully revived, and the movement is straightway executed by the passage of stimuli downwards through a corresponding set of internuncial fibres issuing from the kinæsthetic centres in action, and proceeding thereby to the appropriate motor centres.

Now this conception being true, we should only expect (what is in accordance with facts) one set of excitable centres to exist in each cerebral hemisphere for the different voluntary movements an animal is capable of executing, and therefore I contend that existing evidence points strongly to the conclusion, that the centres in the Rolandic and marginal areas are, as I have termed them, kinæsthetic and not motor.

Did motor centres really exist in the cortex, clearly there should be a double set of excitable centres in each hemisphere—the one set corresponding with the definite points whence fibres pass off from the kinæsthetic to the motor centres, and the other set at the several motor centres themselves and the points whence their efferent fibres issue.

If it should be said by some, as by Dr. Ross and Mr. Horsley, that muscular sense centres may be situated in the upper layers, and corresponding motor centres in the lower layers of the same regions of the cortex, the difficulty of the absence of the two sets of centres in each hemisphere would certainly be got over. But I fully believe and hope that the persons who adopt this view may, after a time, come to see that there is really no reason for postulating the existence of motor centres in the cortex at all, when they have rejected the doctrine by which alone any such view was rendered necessary—the notion, that is, that psychical processes are direct accompaniments of the action of motor centres, and therefore that such centres would need to be correlated in the cortex with sensory centres and their derivatives. Once get rid of the notion, that any such psychical processes exist in connection with the activity of motor centres, as Dr. Ferrier and Dr. Ross have done, and there is no longer the faintest need for postulating the existence of cortical motor centres.

Critical Digest.

SOME RECENT OBSERVATIONS ON PERIPHERAL NEURITIS.

BY R. H. PIERSON, M.D. (PIRNA, DRESDEN).

SINCE the publication of Leyden's paper on Poliomyelitis and Neuritis, in 1880, which was founded on some observations made by Duménil, Eichhorst, and Leyden himself, the number of cases of peripheral (multiple) neuritis related by different authors has become so considerable that we must necessarily abstain from attempting an exhaustive review of the entire literature of the subject; we shall try, however, to give a general survey of the question as it appears in some more or less elaborate works of recent date.

To begin with English work, BUZZARD's monograph: "On some forms of paralysis from peripheral neuritis," treats the subject with all the accuracy and thoroughness that characterises this writer. The little book is divided into three lectures, the first of which contains a more general account of the various forms of neuritis, such as localised forms of neuritis (traumatic, rheumatic); typical (multiple) neuritis; alcoholic, diphtheric, syphilitic neuritis; neuritis from gout; endemic neuritis (kakké or beriberi). Dr. Buzzard justly insists upon the importance of a thorough examination of the electrical reactions in all cases where the presence of neuritis must be kept in view, and he shows in several instances the diagnostic value of this investigation. It is interesting to find here the statement, that a most precise description of typical multiple neuritis (observed in Paris) has been given forty years ago by Graves, in his "Clinical Medicine." He also states that the French pathologists "searched anxiously in the nervous centres for the cause of this strange disorder, but could find none. There was no evident lesion, functional or organic, discernible in the brain, cerebellum, or spinal marrow."[1]

[1] I have no doubt that cases of multiple neuritis might be found under different names in the literature of former years; so I came across an observation mentioned by Prof. Meynert, of Vienna, in a paper called 'Skizzen über Umfang, etc., der klinischen Psychiatrie,' p. 22 (Vienna, 1876): a waiter of most intemperate habits, having caught cold by walking on cold stones at night, complained of violent pain in the legs; is unable to walk (ataxy); paresis of the hands ensues; faradic excitability of muscles diminished; facial paralysis on the right side, dilation of left pupil, articulation impaired, total loss of sensation in both legs; death 55 days after onset of paralysis. Autopsy: Brain and spinal marrow show nothing but slight atrophy of brain and a small degree of hydrocephalus.

In the second lecture we find an accurate description of the causes, symptoms, and anatomy of multiple neuritis, more especially the alcoholic and the endemic forms of this disease. Dr. Buzzard believes that Déjerine's cases of "névro-tabes périphérique" must be considered as specimens of alcoholic neuritis, and he points out the difficulty which may occur concerning the distinction of such cases if ataxy, impaired sensibility, pains, absence of knee-phenomenon, etc., form real tabes.

The third lecture is devoted to the rarer forms of multiple neuritis, which sometimes occur after enteric fevers, dengue and malarias, and the well-known diphtheritic paralysis, which Dr. Buzzard considers to be due, in most cases, to multiple neuritis. Then follow some valuable hints about the diagnosis and prognosis of the different forms of polyneuritis, to which are added directions for the treatment of the disease.

Dr. Buzzard's book fully deserves to be studied by all those who are interested in the progress of neurology.

Of the great number of articles on neuritis published in archives and journals, we can only mention a few. Roger[1] gives a survey of a considerable number of cases observed by several authors, adding a case of his own. This author attempts to identify Landry's acute spinal paralysis with Duchenne's paralysie ascendante subaiguë. He says that in the former the progress of the disease is too rapid to allow the development of muscular atrophy; the absence of sensitive troubles in these cases, as in Duchenne's disease, he considers as unimportant for the diagnosis, and he distinguishes therefore two forms of subacute polyneuritis according to the presence or absence of sensitive disturbances. We do not think that the present state of our knowledge justifies this statement; though since Duchenne's, Landry's, Westphal's, &c., publications on the subjects, the microscopical anatomy of the nervous centres has made considerable advances, this question may not yet be decided; from the clinical standpoint sensory symptoms are essential for the diagnosis of neuritis; and the mere fact that in some cases of Landry's paralysis no alterations of the spine and brain have been found, is not sufficient to make us look upon them as specimens of polyneuritis.[2]

[1] "Des névrites périphériques;" 'L'Encéphale,' 1885.

[2] That this view is not correct I can show by a case of Landry's paralysis which I observed in Dresden, in August 1884. The patient, an unmarried lady 47 years, had been ill about five or six days, when I was requested to see her. Dr. Hagspihl, her usual medical adviser, told me then that paralysis of both legs had been established within a few days without apparent cause. There was no fever, but great mental excitement. The patient could not move the legs at all; she could not sit without difficulty; there was not the slightest trace of muscular atrophy in the paralysed muscles; both tendon-reflexes and deep reflexes were normal; by a careful electrical examination I could not find any alteration of the faradic or galvanic reactions. Within the ensuing three or four days the upper extremities became paralysed; there was difficulty of breathing, sleeplessness, great agitation. I then ceased to attend myself; but my friend, Dr. Mossdorf, who took charge of the patient, told me that he repeatedly tested the electrical reactions without finding any change, nor was there any atrophy of

An observation published by Strümpell and Möbius[1] deserves special attention, because it proves the existence of exaggeration of the tendon-reflexes in peripheral neuritis. (Buzzard, too, has noticed this symptom in cases of alcoholic neuritis.) The authors found, in two patients, exaggerated tendon-reflexes in both arms and legs, in connection with muscular atrophy, partial R. D., and other indubitable symptoms of polyneuritis; as the patients began to recover, the tendon-reflexes became generally normal. Strümpell and Möbius are inclined to explain this phenomenon by assuming a state of increased irritability in the sensory nerves of the muscles and their neighbourhood; this is the more probable, as in both cases other sensory troubles, pain, anæsthesia, etc., were most prominent symptoms.

A great number of authors have written on the interesting form of neuritis which is not uncommonly observed in intemperate individuals. As a most accurate and elaborate essay on the subject, we have to mention Professor Bernhardt's paper, "Ueber die Multiple Neuritis der Alkoholisten."[2] After describing a characteristic case of his own observation, Professor Bernhardt gives a detailed account of a considerable series of cases, mentioning more especially the articles of Rich, Schulz, Strümpell, Müller, and Moeli. The author then describes the recent observations of alcoholic ataxy, or "Pseudo-tabes," and their relation to tabes dorsalis; finally he treats the differential diagnosis of multiple neuritis, tabes, subacute poliomyelitis, and Landry's paralysis. When Professor Bernhardt, quoting the well-known cases of Eichhorst, Roth and Broadbent, says that we are entitled to state that after abuse of alcohol there has been observed a form of paralysis, rapidly ending by death, and likely to make the impression of Landry's paralysis even upon an experienced observer, we cannot deny this to be so; but we cannot admit that this form of paralysis has been proved to be dependent upon multiple neuritis until this will have been shown by post-mortem examination of the peripheral nerves in a case of this kind.

Francotte[3] has observed four cases of multiple neuritis, two of which ended fatally; one of them was that of a patient suffering from pulmonary consumption, in the other case there was no cause at all to be found. Microscopical examination showed in both cases

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the muscles. The patient died suddenly three weeks after the onset of the disease, in consequence of respiratory paralysis. The autopsy was made by Dr. Birt-Hirschfeld, now professor of pathological anatomy at the University of Leipzig. There was no macroscopical alteration in the medulla spinalis; Prof. Birt-Hirschfeld intended to submit the medulla to microscopical examination, but I have not heard what the result has been. At all events, this case proves that *Roger's* suggestion, that the development of the disease is "trop rapide pour que cette altération (muscular atrophy) ait le temps de se produire," is not acceptable. I may add that in my case there was not the *slightest sensory trouble* up to the very death of the patient, nor was there paralysis of the sphincters of the bladder and rectum, bed-sore or any of the symptoms of acute myelitis.

[1] 'Münchener Med. Wochenschrift,' 1886.

[2] 'Zeitschrift für klin. Medicin,' 1886.

[3] 'Revue de Médecine,' May 1886.

that there was degenerative atrophy of the nervous substance, more developed in the peripheral parts; the anterior roots and the spinal cord being perfectly normal.

The two other cases recovered; one of the patients was a drunkard; the other, a prostitute, had no symptoms of syphilis.

Kast, of Freiburg, has published a very interesting article in the Archiv für klin. Medicin' (vol. 40, I.). One of his cases is remarkable for its etiology; the patient, a girl 13 years of age, having had a very slight sore-throat (angina follicularis) which was followed by paresis of accommodation, ataxy, first of the arms, afterwards of the legs, impairment of tactile sensibility, shooting pains; then ensued atrophic paralysis of the interossei and of the tongue with R. D., loss of tendon-reflexes, bulbar symptoms; death nine months after beginning of the disease; multiple degeneration of peripheral spinal and cervical nerves, brain and cord normal.

The second one was a man of intemperate habits; the symptoms of multiple neuritis were combined with swelling of several joints. The patient recovered partially. Kast considers this polyarthritis as a symptom of polyneuritis, and agrees in this respect with Strümpell. There is a considerable number of cases on record in which painful swelling of joints occurred either in the course of multiple neuritis or preceded the outbreak of the latter. Two observations of this kind have been made in 1883 by Boeck, of Christiania; one of them was combined with purpura rheumatica. Boeck suggested that this latter symptom, as well as the swelling of the joints, is due to an affection of the vasomotor nerves, caused by the same infectious agent which produces the neuritic process in the motor and sensory nerve-fibres. This explanation would be apt to account for those cases where the symptoms of acute polyarthritis seemed to appear, as it were, alternately with polyneuritis, as it was observed by Grocco and Fusari,[1] and several other authors.

We add, finally, that the statement, first made by Baelz and Scheube, that the endemic disease of beriberi or kak-ké is due to multiple peripheral neuritis, has been confirmed by Tscholowski, of St. Petersburg, who examined, post-mortem, several cases of this disease, and found well-marked degenerative atrophy of peripheral nerves together with slight atrophy of some ganglion-cells in the lumbar region of the cord, which latter alteration he is inclined to consider as a secondary occurrence.

[1] 'Annal. univers. di Med. e. Chirurg.,' 1885.

Abstracts of British and Foreign Journals.

Landouzy and Déjérine on Progressive Atrophic Myopathy involving the Face. (*Revue de Médecine*, Dec. 1886.)—The authors have devoted much time and trouble to the very careful and minute investigation of the various types which are to be found in the disease which is commonly called Progressive Muscular Atrophy (*Comptes-rendus de l'Académie des Sciences*, Paris, 7 Janvier 1884. *Revue de Méd.*, Feb.–April, 1885). They consider that the types, at least which they describe, belong to a disease beginning "in" the muscle and not in the spinal cord, a conclusion they wish to indicate by calling the disease a progressive atrophic myopathy, instead of a progressive muscular atrophy. The types which they claim previously to have established are: (1) the facio-scapulo-humeral type, and (2) the scapulo-humeral; of which the second was believed to be much the more common, and the first was considered to belong to only a very few cases originating in childhood, such cases, in fact, as Duchenne had called Progressive Muscular Atrophy of Children. The chief points they wish to add to what they have said before are, that the affection of some facial muscles is much commoner than was supposed, that it consists chiefly in thickened lips, the upper generally overhanging, along with weak and unequal movements of the corners of the mouth and of the cheeks, and an incapacity to shut the eyelids completely, which gives this *facies myopathica* a heavy look and clumsy movements more easily recognised than described. The pronunciation is very indistinct, especially of the labials. And this *facies* they regard as a factor, which may come first in the symptoms of the disease either in children or adults, or may come on gradually late in the disease, or may possibly never enter into it at all. The muscular lesion which manifests itself by atrophy they consider as of the same nature as that which manifests itself by pseudo-hypertrophy; they both originate, as the most modern researches are taken to show, in a muscular irritation. That the clinical results of atrophy and pseudo-hypertrophy should be so distinct is a parallel to the cases of interstitial hepatitis, which are sometimes from beginning to end atrophic, and sometimes from

beginning to end hypertrophic. They bring forward six new cases in full detail, and in one of these was an autopsy. The first subject was a man of 36, whose grandmother, mother, and brother were atrophic, and in whom the inherited *facies myotrophica* was plain from childhood. His playmates called him a "Chinaman." He could not keep his place in a military band, because there was no chance of his ever learning to play the flute—his lips were too clumsy and hypertrophied. His scapulo-humeral symptoms on the left side were well developed at the age of 15. He was allowed to stay in the army till after the Franco-German war, when he was 21, though he only pretended to shoot from the left shoulder, and after that could do very little. He was treated in the Charité in 1885. By good fortune there were photographs of his family and himself, which showed the same type of face and atrophy in them, and the progress of the same disease. The photographs of his own case are admirably reproduced in the *Revue*. The second case was in a woman æt. 27, who came at first to the Hospital, not imagining she was ill herself, but entirely as interpreter for her paralysed mother, and in her the disease was recognised from her face only. She seemed entirely unconscious of any abnormality either in the face or the arms, but on examination the ordinary scapulo-humeral type of atrophy was found; and photographs showed that her facial symptoms had existed from childhood. She could not whistle, or blow out a candle, or frown, or shut her eyes completely. Her lips were thick, and moved very little in speech or laughter; there was reaction of degeneration in the *orbicularis oris*.

The third subject was a man, born in 1856, in whom some affection of the face was noticed when he was three years old. Some atrophy of the muscles of the trunk and legs came on two years later, and very gradually spread to the arms. When he was nine he was taken to see Duchenne at Boulogne, who was very much interested in the boy, as illustrating that rare form of disease which he had then (1868) named atrophic paralysis of children. He published an account of the case (*De l'Electrisation localisée*, 3me éd. 1872, p. 1098), fully describing the facial symptoms mentioned in the other cases, except the inability to close the eyes completely; he illustrated them by photographs, and found similar symptoms in his mother and his brother. A year or two later the flexors of both legs began to contract, and he became a hopeless cripple. He could use his hands, however, and made a scanty living by being dragged about France in a go-cart by an idiot, and selling little

books of instruction in shorthand writing. At last the idiot died and he came into Hospital under the care of M. Landouzy in 1886, showing the same *facies myopathica*, of which Duchenne had published a description 18 years before, with the additional inability to close the eyes completely which had not been noticed previously. The atrophy in the arms and neck were much more advanced. In the 4th case there was no history of heredity; atrophy of a scapulo-humeral type came on æt. 40; and no affection of the face was noticed till he was 45. In the 5th case there was probably some complication with lead-poisoning. He was a man of 40, who had had dropped wrist in 1872, and in consequence given up his trade as painter; but in 1885 was found to have progressive atrophy of scapulo-humeral type, with the addition of facial symptoms in 1886 and notable atrophy of the tongue. The 6th and last case, in a man of 66, was fatal, and was examined very minutely. At the age of 20 he began to notice atrophy about the shoulder, which spread very slowly into the arms and legs. He was a hawker, and could go on with his trade till he was 63. There was no visible affection of the facial muscles whatever; no fibrillar contractions; no reaction of degeneration; no abnormality of knee-jerk and no hereditary history. He died of pulmonary-tuberculosis, and an autopsy showed no abnormality of the spinal cord, of the cervical sympathetic, of the anterior roots of the peripheral nerves; but simple atrophy of the muscular fibres with great multiplication of the nuclei. In some muscles a few fibres were hypertrophied, and in the most atrophied muscles there was some interstitial fat. The point on which the authors lay most stress is that, though they could not detect abnormality of the facial muscles in life, they found after death that they were degenerate or beginning to degenerate, a point they consider of importance as showing that, sooner or later, the face becomes involved in this progressive atrophy. Westphal has also published three cases of progressive muscular atrophy with affection of the ocular muscles (*Charité-Annalen*, Berlin, 1886), and he notices the thick, overhanging upper lip, "like a tapir's," in one of them.

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